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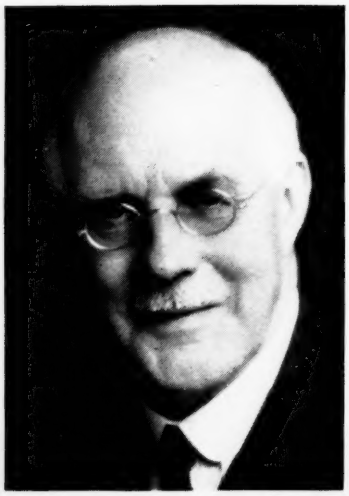

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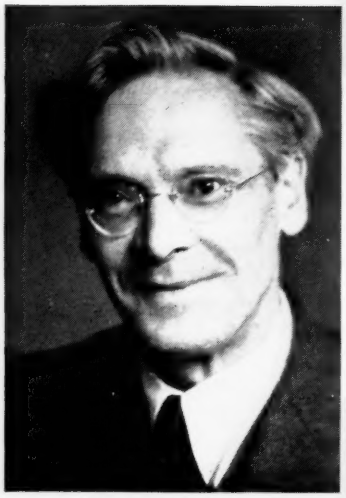

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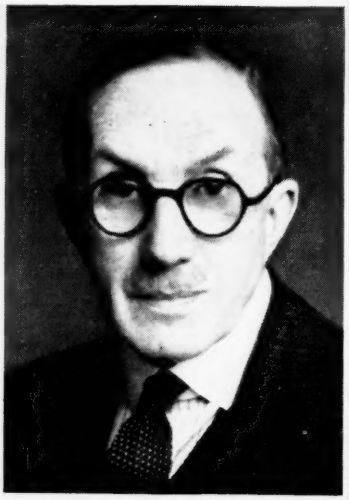

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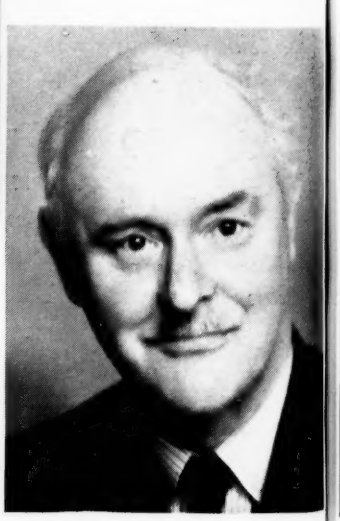

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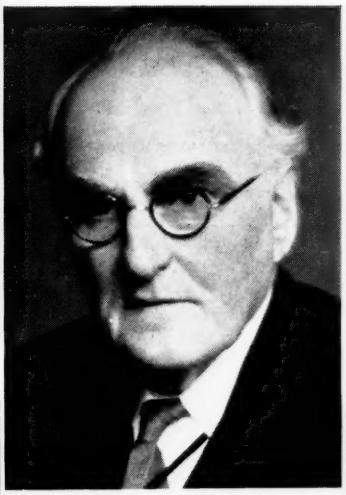

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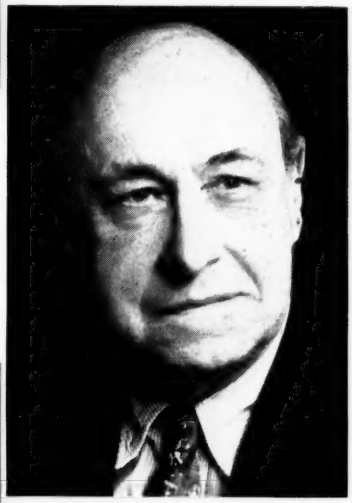

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The portrait of Prof. H. D. Kay is by courtesy of the Society of Dairy Technology.

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MAN'S USE OF ENERGY

Address by

SIR HAROLD HARTLEY, K.C.V.O., C.B.E., M.C., F.R.S.

PRESIDENT OF THE ASSOCIATION

FOR the sixth time the British Association is enjoying the hospitality of Birmingham, this great centre of industry and learning. It is a happy coincidence and a special pleasure to us all, Mr. Vice-Chancellor, that we are meeting here in the Jubilee Year of your University. May I offer you on our behalf our warmest congratulations and good wishes. Although your University is young, as Universities go, you inherit the traditions of learning and culture that have flourished in Birmingham ever since the days of that brilliant galaxy of talent—Darwin, Galton, Boulton, Watt, Priestley and their friends—the Lunar Society. If we needed any reminder of the great part that Birmingham played in the Industrial Revolution, we have it in that admirable *Scientific Survey* for which we are all most grateful to the authors. It was here that the genius of James Watt found at long last the means and opportunity to complete his engine, that decisive step in the history of industrial progress.

What better setting could there be for the subject of my address to you to-night: Man's Use of Energy. It seemed to me the natural sequel to Sir John Russell's address to you last year, and a fitting remembrance of the centenary of that landmark in the history of science, the Second Law of Thermo-dynamics, the law that governs the conversion of thermal energy to work of use to man.

It is only a century since man first understood the full import of the word 'energy' and its significance as the driving force of all physical and chemical change and of life itself—what Clerk-Maxwell called 'the go of things'; and yet it has been man's use of energy that shaped so largely the material progress of the human race. Among the milestones of that early progress were the discovery of fire, the raising of crops, the invention of tools, weapons, the wheel and machines, the copper, bronze and iron ages, and the use of animals, wind or water as sources of power.

Throughout the ages the dependence of the human body on food and warmth, and the demands of man for energy, have helped so much to shape the course of history. Even in this machine age of civilisation, with the huge demands of industry and transport for heat and power, in Britain at least one-third of the energy consumed goes to satisfy our personal needs of heat and warmth and food. And so man's own needs, the energy by which life is sustained, will be my starting point to-night.

ENERGY AND THE HUMAN BODY

Why does the body need this energy? Just as a ball of celluloid poised on a jet of water from a fountain will keep its place and spin so long as energy is there, so life depends upon that flow of energy. Even in sleep the muscles of the heart and lungs must maintain the circulation of aerated blood to meet the body's need of energy through the chemical reactions on which the steady state of living matter rests. We know from radioactive atoms, by which the movements of matter can be traced, that all parts of the body, even the bones, are being constantly renewed.

And then there are the muscles on whose movements life in turn depends. The intimate mechanism by which they derive their energy, their power to work, by chemical reactions from the intake of food and oxygen is still one of Nature's secrets. It is a mechanism much more subtle than the internal combustion engine as it works at constant temperature. The outstanding fact about

the human body is that all its manifold changes and adjustments, thanks to Nature's catalysts, take place at the steady temperature at which evolution has decided that the body should work.

Comfort and life itself depend on that constant body temperature. Clothing, domestic heating, air conditioning are all designed to meet the body's thermal needs in its environment. The body mechanism can respond to a demand for greater heat production or its dissipation, but the demands on it are least in temperate climates with no extremes of heat or cold. Markham following this clue in his book *Climate and the Energy of Nations* points out that the earliest civilisations were in the warm fertile regions of the Middle East and Southern Mediterranean. As the art of living improved with housing and domestic heating, dominance passed to the temperate climates, where it has since remained. Birmingham, I am glad to say, falls within Markham's zone of optimum conditions for man's creativeness.

But let us return for a moment to the destination of the energy consumed by the body when asleep. If fatigue is of the nature of some disturbance of the steady state, some of the energy may go to readjustment, to nature's running repairs. It is significant that no less than a quarter of the intake goes to the brain, even when the organs of external sensation are at rest. It seems an enormous consumption for its weight, but it is maintaining the active state of the ten thousand million nerve cells of which the brain is composed. We speak of grey matter or the lack of it, and the grey matter consumes two to four times the energy needed by the white.

Why does the brain need this great expenditure of energy? Probably because it is the integrating unit, the control room, on which depends the marvellous efficiency of the body, the accurate co-ordination of sense and action. And the memory, that vast storehouse of impressions, must be kept alive like the automatic memories of the electronic calculators, but with an immeasurably greater efficiency. Even in sleep the subconscious mind is working with the flow of energy, and who can say how much we owe to it? In our researches we can plan our measurements and observations, accumulate new facts, but the flash of inspiration that marks the great advances comes from the 'deep wells of unconscious cerebration'; it is not at our command.

Life is dynamic, and the slender thread on which it hangs is that ceaseless flow of energy.

I must not dwell longer on these fascinating unsolved problems of the power engineering of the body and the human brain, or the mechanisms by which energy performs its tasks. Gilbert Lewis once asked if living things 'were cheats in the game of entropy?' Does Nature even here obey her laws of energy? This we may never know for certain, but the presumption is—she does.

Man's Progress in the Art of Living

Man's progress in the art of living, the environment he has created for himself with its ever increasing call for energy, are the products of the human brain. Its curiosity, its creative ingenuity, its ability to observe and take advantage of the unexpected, reflect its gradual evolution through man's efforts to improve his lot.

For a million years man was 'the servant of untamed Nature,' and only when he learned to tame her to his needs, to kindle fires and grow his food, did the social evolution, that made us what we are, begin. 'Man,' said Carlyle, 'is a Tool-using animal. Weak in himself and of small stature, he stands on a basis, at most of the flattest-soled, of some half square foot, insecurely enough . . . Three quintals are a crushing load for him: the steer of the field tosses him aloft like a waste rag. Nevertheless he can use Tools, can devise Tools . . . without Tools he is nothing, with Tools he is all!'

The story of material progress is man's invention of tools to make his muscular work more effective, or to store it and raise its potential as with the bow and arrow, his invention of machines to multiply his force and ease his tasks, or to repeat them with accuracy independent of the hand or eye. Animals, wind or falling water gave him the energy he needed to multiply his effort. The successive metal ages, copper, bronze and iron, mark man's discoveries of new materials from which to make his tools and weapons and machines, and his

inventive use of energy in fuels and furnaces to smelt their ores.

The Industrial Revolution

Even in those far-off days the name of engineer reminds us of the impetus that came in use of energy when man's mind was focussed on the military art. Nevertheless progress was slow, with little help but trial and error save for a brief period among the Greeks, until the new freedom of the mind that came with the Renaissance and its spirit of enquiry taught man to question Nature by experiment and to read her answer. That was the birth of modern science. In his *New Atlantis*, Francis Bacon made a prophetic forecast of the spirit and the optimism of the Scientific Age—' The end of our foundation,' he said, ' is the knowledge of causes and of the secret of things, and the enlarging of the bounds of human empire to the effecting of all things possible.' At the same moment the experimental study of mechanics by Galileo was yielding a fresh understanding of machines and quickening the tempo of invention. Here in Britain, with the impetus that social freedom gave to industry, with the momentum of the Elizabethan age, and with our native genius the times were ripe for change. It was here that the machine age, the Industrial Revolution, with its dependence on energy, began. It was a great century of British invention. In textiles, Kay's flying shuttle, Hargreaves' jenny, Arkwright's frame and Crompton's mule led the world. Darby's discovery of smelting iron with coke instead of charcoal and Cort's reverberatory furnace and rolling mill for making wrought iron brought the blast furnaces and forges northwards to the coalfields from the woodlands of the South. Not all the improvements in machinery came from the users and makers of machines, but 'some,' as that shrewd old observer Adam Smith records, ' by the ingenuity of those who are called philosophers or men of speculation, whose trade it is not to do anything, but to observe everything ; and who, upon that account, are often capable of combining together the powers of the most distant and dissimilar objects.' So science played its part.

It was a time of rapid growth, and with the advent of the factory and machines, supplies of power from water-wheels and animals were soon the limiting factor. For fifty years Newcomen's atmospheric engine had been the only means of draining mines as they went deeper to meet the growing needs of coal and iron, and no improvements had been made in its efficiency of one half of one per cent.

Watt's Steam Engine

Then in 1764, by one of those chances that make history, a model of the engine went to a young Glasgow instrument maker to be repaired. It stirred in the mind of James Watt a restless urge of his inventive genius and his intuition for mechanical design. Watt wasn't satisfied ; he saw the waste of steam and instinctively he attacked the problem by the scientific method, by measurement. He measured the boiling point of water at different pressures, and the volume of steam formed from a volume of water : he made a model to show the quantity of steam used at every stroke of the cylinder. He was puzzled to find that steam could heat six times its weight of water from room temperature to boiling point, which Black explained to him by his discovery of latent heat. It was not until the following May that in a flash of imagination on a walk, Watt saw that the waste of heat could be avoided by condensing the steam in a separate vessel, the condenser of the modern power unit. As he said ' I had not walked farther than the Golf House when the whole thing was arranged in my mind ! ' A hastily made model, of which a thimble, presumably his wife's, was part, satisfied him that he was right, but it was ten years before the first engine was made to his design here in Birmingham, marking the birth of a new age of civilisation.

Watt had now to earn his living as a surveyor, and despite the help of friends and his partner Roebuck, the engine made slow progress. In 1768 he rode to London, took out a patent, and on his way north he came to Birmingham. Here Matthew Boulton with the ambition, the enthusiasm and the vision of a great industrial venturer had just built the Soho works, the prototype of a modern factory. Watt saw there

the opportunity to build his engine, while Boulton's quick eye saw the engine meeting the needs of industry for power. Watt's partner offered Boulton a licence to build engines for the Midlands, but Boulton's mind was moving on a grander scale and his answer reveals the wide sweep of his imagination. ' It would not be worth my while,' he said, ' to make for three counties only, but I find it very well worth my while to make for all the world.' And so for six years Boulton waited till Roebuck's bankruptcy gave him the opportunity he sought, and then Watt came to Birmingham as his partner with his engine still unfinished. Within a year the first two engines were erected, the first at Bloomfield Colliery in Staffordshire and the next at John Wilkinson's new foundry at Broseley to raise the water to drive the blast. Their success owed much to Boulton's encouragement and help and to the accurate cylinders made by Wilkinson's new machine tool with a limit of error not exceeding ' the thickness of a thin sixpence ' in a diameter of 72 inches.

The immediate market for the Watt engine was to drive the pumps in the Cornish mines with an efficiency four times that of the Newcomen engine. Watt went himself to erect them until another Scottish inventor, William Murdock, walked from Glasgow to Birmingham following Watt's footsteps. Watt was in Cornwall, and the interview with Boulton was not going well when Murdock's hat fell on the floor. Boulton noticed the noise. ' What's it made of?' he asked. ' Timmer, Sir,' said Murdock, ' I turned it myself on a bit lathey of my own making.' Boulton recognised the skill and knowledge that had gone into the making of a lathe that could turn an oval, and it was not long before Murdock was building Watt's engines in Cornwall and settling arguments with the miners with his fists. And it was there that he invented the first gas lighting for his home in Redruth and built the first model of a steam-driven carriage, that the vicar on a dark night mistook for the devil himself. While Murdock was erecting the engines Watt was back in Birmingham busy with successive improvements to add to those he had already made—the tubular condenser, the steam-jacketed cylinder, the double-acting piston, expansive working, parallel motion, the governor, and the indicator to measure the work done in terms of horse power, Watt's unit that we still use to-day.

' Invention is the mother of necessity,' and Boulton realised that the machines that were being invented in quick succession needed rotative power to drive them. ' The people in London, Manchester and Birmingham are steam mad,' he wrote to Watt urging on him the need to take out a patent for ' methods of producing rotative motion from the fire engine.' Watt's cautious nature was intent on the success of his Cornish engines, and Boulton persuaded Murdock not to go to London to patent his steam carriage lest it should divert his energies. The first rotative engine was built in 1782 with Murdock's sun and planet gear. It was an immediate success, and by 1800 some 500 engines in all had been built by Watt and Boulton with a total output of 7,500 h.p., making together $\frac{1}{20}$ of the output of a 100 megawatt generator of to-day.

Murdock's invention of gas lighting, like his steam carriage, was never patented. Soho was the first factory to be lit by gas, and it was illuminated by gas to celebrate the Treaty of Amiens in 1802. The gas-making plants made in Soho were being installed in textile works when George Winsor came to London with a scheme for distributing gas to private houses and for street lighting, the first plan for the distribution of energy. The absence of a patent weakened Murdock's opposition to the monopoly Winsor sought, and in 1812 the Gas Light & Coke Company got its charter, but the gas engineer who made it a success was Samuel Clegg who had been trained by Murdock at Soho. So Birmingham is the first home of the gas industry as well as of the steam engine.

1800–1850

Watt's patent ended in 1800 and the field was open to Trevithick's high-pressure engine with its greater efficiency. Progress at first was slow ; even in Birmingham there were only sixty engines with a ' total steam-power in horses ' of 1,000 by 1820. But in the coal mines and the

blast furnaces steam power was indispensable to the production which was soon to make Britain the ' workshop of the world.' Sadi Carnot saw this so clearly in his classic essay on 'The Motive Power of Heat.' ' To rob Britain of her steam engines,' he wrote in 1824, ' would be to rob her of her coal and iron, to deprive her of the sources of her wealth, to ruin her prosperity, to annihilate that colossal power.'

In the cotton mills and steamships also steam power was forging ahead. Fulton's Clermont with her Soho engine was the prototype in 1807 of the steamboats which were to open up America long before the railroad. But it was the triumph of Stephenson's Rocket in 1829 that heralded the new era, the harnessing of energy to production and transport on a scale that made possible the great expansion of the nineteenth century. In the factories steam-driven machines increased continuously the productivity of labour, and the rapid growth of the metal and chemical industries was dependent on supplies of heat and power. The steamship routes and the railways, the new arteries of commerce, quickened the flow of goods and men, and made accessible the raw materials of virgin lands and their new markets for industry. In the economic histories of these times there are statistics of every kind, except the growth of horse power, without which that great expansion of human effort could not have been. But coal, the source of heat and power, is an index of development. In 1700 the output of coal in Britain was 3 million tons or half a ton per head of population, in 1800 we used twice as much per head, but by 1850 we were using 60 million tons or three tons per head. In these years of rapid growth, practice, as so often happens, outran theory. There was no theory of the conversion of heat to work. It was an empiric age, when even Thomas Tredgold described the calculus as a device ' for forcing the assent rather than by convincing the judgment.' Until 1850, as L. J. Henderson used to put it, ' The steam engine did more for science, than science did for the steam engine.' But then a century ago, came swiftly the idea of energy, and the recognition of its laws, that great divide in engineering between the old and new.

The Laws of Energy

Carnot, a young French engineer, realising the importance of a clearer understanding of the working of the steam engine and its complexity, simplified the problem by his conception of an ideal engine working in a reversible cycle. His stroke of genius in recognising that the efficiency of his ideal engine must be independent of the working substance was the first step towards a general theory of energetics. He was the first to show that the efficiency of a heat engine depends on the temperature drop between the cylinder and the condenser. His forecast of the internal combustion engine reveals his clear grasp of the engineering principles that are involved. If he had not died in 1832 his notebooks show that he would have anticipated much that came after him. As it was his ideas had to wait a quarter of a century for recognition.

Next came Mayer, a young German doctor, who noticed when he bled his crew for fever on their arrival in the East Indies that their blood was redder than it would have been at home. He decided rightly that this was due to the reduced demand for heat to maintain the body temperature in the tropics. This set him thinking on the whole problem of the causal relationship of what he called the imponderables—work, heat and electricity—and led him to the theory of the interconversion but indestructibility of these various forms of energy. Once when he was asked how he became involved in this problem, he replied ' Engaged during a sea voyage almost exclusively with the study of physiology, I discovered the new theory for the sufficient reason that I vividly felt the need for it.' He calculated in 1842 the amount of work equivalent to a unit of heat and pointed out the inefficiency of the best steam engines of his day.

In 1847 Helmholtz, then a young army surgeon, from his study of metabolism, animal heat and muscular work was led along the same path as Mayer to consider what the relations must be between the different forces of nature, heat, electricity, magnetism and chemical affinity, if perpetual motion is impossible. And from a masterly analysis he reached the conclusion that ' Nature as a whole possesses a

store of energy which cannot in any wise be added to or subtracted from : the quantity of energy in inorganic nature is as eternal and unalterable as the quantity of matter.' The effect of this new principle of the Conservation of Energy, the First Law of Thermodynamics, was to divert physical research into an entirely new channel. ' Its object,' said Hertz, ' was henceforward to refer all phenomena in the last resort to the laws which govern the transformations of energy.'

Meanwhile Joule was attacking the same problem in Manchester by measuring the relation of work to heat in a series of researches which for insight, ingenuity and accuracy, with the means at his disposal, are unsurpassed. Joule was a faithful member of our Association which gave him scant encouragement. At seven successive meetings he read papers without any record of discussions. In 1843 his paper on ' The Mechanical Value of Heat ' was read to a chilly audience of six. At Cambridge two years later his paper on the measurement of the heat developed by rotating paddle-wheels in water excited no interest, but at last at Oxford in 1847 William Thomson, fresh from helping Regnault with the first ' steam tables,' and from reading Carnot's essay, saw the importance of Joule's paper on ' The Mechanical Equivalent of Heat,' though he was sceptical of Joule's interpretation. Their meeting led to one of those fruitful friendships and partnerships which this Association has done so much to foster. Looking back too at the conservatism of a century ago and at the personal bitterness and opposition to new ideas that sometimes it engendered, I like to think that in this respect, at any rate, we can claim progress in the more sympathetic and interested hearing that new ideas receive both in this Association and in the world outside.

Even in 1849 Thomson had not shaken off the eighteenth century idea of heat as something invariable in quantity like matter, showing how novel was the new idea of energy. So it was left to Clausius, a young German physicist, in 1850 to reconcile Carnot's theory of the heat engine with the newly discovered equivalence of heat and work and to formulate the Second Law of Thermodynamics in the general statement ' That heat cannot of itself pass from a colder to a hotter body.'

Rankine, a brilliant Scottish engineer, reached the same conclusion from his theory that heat is due to the rotation of molecular vortices. He had the conception of the thermodynamic function which Clausius later called entropy. Rankine's analysis of the energy changes during the expansive working of the steam engine was later known as the Rankine Cycle and became the basis of design of steam prime movers.

Then Thomson recanted, and having seen the light, with a bold sweep of imagination he surveyed all the sources of energy available to man for the production of mechanical work and recognised the universal tendency in nature to the dissipation of energy. From it he drew the conclusion, momentous as it then seemed, that the earth must in time become too cold for human life. Our astronomers may give us a very different answer to-day.

And so, starting from the theory of the steam engine, as the word thermodynamics reminds us, there came a gradual realisation of the common driving force behind all physical and chemical changes, and of the laws of energy which govern them. Those laws, independent of hypotheses as to the nature of matter or of process, in the hands of Helmholtz, Willard Gibbs, van't Hoff, Planck and Gilbert Lewis, have become a universal instrument of great power and great economy in predicting what is possible. The height of a blast furnace was once doubled at great cost to see if the reducing power of its gases could be increased. Thermodynamics could have given the negative answer on a sheet of notepaper.

By 1850 industrialisation and the use of power was spreading from Britain to other countries with great demands for energy. Between 1850 and 1900 the world's coal consumption increased from 100 to 1,000 million tons a year and the route mileage of the world's railways grew from 25,000 to 600,000 miles. It was a time of rapid change when the modern interplay of science and technology was taking shape and invention followed quickly on necessity. The steam engine made great progress in efficiency and size to meet the growing need of power. Double, treble

and quadruple expansions followed one another to get the maximum amount of useful work from the higher-pressure boilers. Designers were slow at first to profit by the new theory and improvements came mainly from details of mechanical design.

1850–1900

Two new ventures in the field of energy, were destined soon to change the technical and social pattern of the world as steam had done a hundred years before. Since Murdock's day the gas and hot-air engines had been a favourite field for inventors, until with the work of Lenoir, Rocha and Otto the gas engine became a practical reality in the seventies, and the supply of town's gas made it a convenient source of small-scale power. Carnot's cycle gave it promise of greater efficiency than steam and scientific enthusiasm was on its side. At the Jubilee Meeting of our Association at York in 1881, Sir Frederick Bramwell, speaking of the transmission of power by gas, predicted that in fifty years the steam engine might be spoken of as a curiosity to be found in a museum ! Prophecies to this Association that are a challenge to the technician are apt to be belied, and it was only three years before Parsons' turbine rescued steam from its predicted fate. However, Bramwell's optimism about the gas engine was justified, though not as he foresaw. In larger units its promise of success was not fulfilled, but its progeny, the petrol engine and the diesel, are to-day in aggregate the greatest power producers in the world. The sinking of the first oil well in Pittsburg in 1859 saw the birth of a great industry, a new source of energy, which in the hands of Diesel and Daimler and many other eager spirits has given us the internal combustion engine of to-day with its far-reaching uses.

Even such an optimist about the gas engine as Fleeming Jenkin soon saw a cloud on the horizon. In 1882 he wrote to Ewing, ' The gas engine goes slowly, its nose is put out of joint by this electrical affair.' For the beginning of that electrical affair we must go back to 1831. Faraday with his instinctive belief in the essential unity of the forces of nature was convinced that electricity could be obtained from magnetism as magnetism had been obtained from electricity. Time and again he experimented without result, until on August 29, 1831, the induced current was detected. Ten days of decisive experiment culminated in his paper on ' The Induction of Electric Currents ' which was to shape the future of electrical science and electrical industry. The discovery of electro-magnetic induction gave a new impulse to Faraday's mind and gave him confidence in the flights of his imagination which inspired his *Experimental Researches in Electricity*. And these in turn, translated by Clerk-Maxwell into mathematical form, led him to the electro-magnetic theory of light and to the conception of electro-magnetic waves, which in the hands of Hertz became the basis of wireless.

Faraday's discovery set the inventors' minds working and one of the first uses of electro-magnetic machines was in electro-plating. In 1845 Faraday visited Messrs. Prince's works here in Birmingham to see the Woolrich generator at work. ' I shall never forget,' wrote John Percy, ' the sparkling delight which he manifested on seeing the result of his purely scientific labours, rendered subservient to a beautiful art and to the advantage of others.'

In 1861, foreseeing the need for universal use of a system of electrical units based on certain fixed standards, the British Association appointed a strong Committee to investigate the subject. The system of units based on the centimetre, the gram and the second adopted by this Committee and the standard ohm constructed by them were subsequently adopted internationally with minor changes. In this way the Association rendered an outstanding service to the progress of electrical science and engineering.

By 1870 with the dynamos of Pacinotti, Siemens and Gramme the generation of electric current was reaching an economic stage and its use for lighting by the arc was growing. But it was the invention of the carbon filament lamp by Edison and Swan that in 1880 gave the first great impetus to the electrical industry. The next landmark was Edison's power station in New York with its D.C. generators providing low voltage lighting current for the neighbourhood. And this was the pattern

of development until Ferranti saw that the future of electricity supply lay with the high-voltage transmission of alternating current generated at some convenient point outside a city. No difficulties daunted his inventive brain, and the construction of the Deptford Power Station with its long high-voltage line to London was due to his creative skill. Alternators, cables and transformers were all made to his design and they ran for 30 years, the prototype of the modern power system.

Meanwhile Parsons' discerning eye had seen that the marriage of the reciprocating motion of the steam engine to the rotation of the generator was an unnatural union. The turbine has been a dream for at least 2,000 years, but with Parsons' genius for design in 1884 his first experimental model came like Minerva fully armed. A century before, Watt summed up his sagacious answer to Boulton on the competition they might have from Kempelen's steam turbine with the words : ' In short unless God makes it possible for things to move a thousand feet a second it cannot do much harm.' Mechanical progress and Parsons' genius had made the impossible possible and with it came that smooth continuous high-speed drive that is the key to so much of modern engineering. Forty crowded years of investigation and experience have scarcely changed the profile of the blading that Parsons' intuition shaped in 1896. As Kirchoff said of Faraday, ' He smells the truth.' With the turbine and the high-pressure boiler, the polyphase alternator with its rotating magnetic field, and the electric motor, steam and electricity entered on a new age of power.

Energy in the Twentieth Century

The last half of the nineteenth century had been a period of continuous expansion. The steady rise in world production, following the law of organic growth, seemed to justify the satisfaction of the Victorians and their optimism about the future. In science too the edifice of classical physics seemed almost complete. But the twentieth century was destined to be a time of rapid and revolutionary changes both in science and technology, in which man's use of energy was to play a decisive part. With the turn of the century a series of discoveries gave a new insight into the nature of matter and of energy and their relationship : radioactivity by Becquerel, X-rays by Röntgen, the electron by J. J. Thomson, the nucleus by Rutherford, the quantum theory of energy by Planck, and relativity by Einstein. Those discoveries changed profoundly man's understanding of Nature's processes and gave him new techniques, new tools, which quickened the pace of scientific advance in every field. Meanwhile the rapid progress of electricity, of the internal combustion engine, and of wireless were creating equally profound changes in the social and technical pattern of the world.

The central power station, hydro-electric plants, and the high-voltage transmission of electricity made power available over wide areas both in the factory and in the home. The distribution of power encouraged the dispersal of industry, it substituted for the steam engine the more flexible electric drive, and it gave a great stimulus to mechanised production. The hundred-fold increase in the world's consumption of electricity between 1900 and 1950 was accompanied by a six-fold increase in efficiency of generation.

The gas industry, foreseeing the inevitable loss of the lighting load to its younger rival, concentrated its efforts on the economical supply of heat and on its by-products. By scientific methods its efficiency has been raised to nearly 80 per cent. In countries that are fortunate enough to possess reserves of natural gas, the ideal fuel, high-pressure mains have been built to carry it great distances. To-day gas provides in all 9 to 10 per cent. of the world's energy requirements.

In 1900 the production of oil was 20 million tons, by 1925 it had increased to 150 million tons and by 1950 to 500 million tons. Vacuum distillation of oil came in with the new century ; by 1930 methods of refining had so progressed that oil could be tailored to meet the needs of the market with its ever expanding demand for high octane petrol. Since then catalytic cracking and the so-called fluid catalyst have set a new high-water mark of efficiency in oil refining.

In 1900 there were less than 20,000 motor vehicles in the world, in 1950 there are 62 million. The coming of the motor

vehicle has made a radical change in social conditions from the new mobility it has given to life in every country.

It was too the progress of the petrol engine, with its six-fold increase in power output from cylinders of equal size, and the reduction of its bare weight per horse-power from 4·5 lb. in 1905 to 1·1 lb. in 1950, which made possible man's conquest of the air and changed the whole relationship of nations by the speed of personal contact it provided. And now with the coming of the jet, men may fly even faster than the waves of sound. But speedier still is wireless which carries the human voice across the world in a fraction of a second whilst television brings to light the distant scene.

Then in agriculture, the replacement of the horse by the tractor and the use of power on the farm have given a new impetus to development. They have increased the output per unit of labour and thus made good the drift of man-power from the land. In England and Wales the total farm supplies of power (excluding transport) rose from just over one million horse power in 1908 to two million in 1939 and five and a half million in 1948. Of this, horses provided 87 per cent. in 1908, 25 per cent. in 1939, and only 7 per cent. in 1948. In the United States of America mechanisation of the large wheat farms has reduced the labour requirements per acre by three-quarters.

In addition the availability of energy has made possible a new partnership between agriculture and industry in the processing of farm products. Canning, refrigeration and deep freezing, conditioned transport, crop drying, fermentation processes, and the conversion of farm residues into useful products have all opened new outlets for the farmer.

And side by side with this new pattern of energy in industry and agriculture has come another change no less far reaching —the Domestic Revolution—the scientific application of heat and power in the home, which has done so much in this century to help the housewife and to raise the standard of health and comfort. It may be regarded as the repayment by the technician of a debt long overdue for some of the social evils of the Industrial Revolution.

What measure have we of the significance of energy to man to-day? In the nineteen-thirties the correlation between the average real income and the consumption of energy per head in different countries was so close, with a few exceptions, that it showed how essential supplies of energy are if a reasonable standard of life is to be maintained. Energy is the multiplier of human effort. But its effects are not found in comfort and productivity alone. The rapid interchange of persons and ideas has brought the world together into a much more closely integrated unit. It has quickened the senses and the nerve fibres of nations and given them a new awareness of events, a new world consciousness which in due time must show their inescapable community of interests.

And with all these new uses of energy and its growing population, the world's demands are rising daily. How can they be met? Sir John Russell's problem last year of the world's demand for food was simple compared with mine. There is a limit to the capacity of the human stomach, but none to man's appetite for energy, and he finds new uses for it every day.

World Consumption and World Resources of Energy

The world's consumption of every kind of energy to-day, food for men and beasts, heat, light and power, needs a gross yearly intake equivalent to four thousand million tons of coal. On a thermal basis this demand is met by food from agriculture 21 per cent., by coal and lignite 41 per cent., by wood 6 per cent., by oil 24·5 per cent., by natural gas 6·5 per cent., by hydro-electricity 1 per cent. So that about 30 per cent. comes from the current revenue of plant and animal growth and water power and the remainder from the capital resources of coal and oil. Leaving aside the work equivalent of human and animal labour, and taking into account the efficiency with which the other energy sources are applied by man, their useful contributions to-day are roughly solid fuel 56 per cent., liquid and gaseous fuel 37 per cent., water power 7 per cent.

What of the future? How long can the world's resources meet these growing demands? The world's reserves of coal

are ample for centuries. Reserves of oil and natural gas are smaller, and harder to define, but fresh fields are being found and there is no reason to fear a shortage for some decades. Before then synthetic oil from coal and the huge reserves of oil in shale will provide substitutes. Turning now from fossil fuels to current revenue, if the world's reserves of water power were all to be developed, their output would equal the total inanimate energy man is using to-day. But these reserves are scattered, and many lie in distant regions far from the crowded areas of the world—in the Belgian Congo, French Equatorial Africa, the Cameroons and South America. And like coal and oil there is a most unequal distribution of resources between nations. The countries north of latitude 20° contain 94 per cent. of the world's coal reserves and 83 per cent. of the oil reserves, while those to the south contain two-thirds of the world's water power potential. So that the pattern of development of countries, which must be related to their energy resources, will inevitably be very different.

But our energy problems of the future are not limited to heat and power alone. Reserves of fertilisers for the growth of food and of metals for the machine age in which we live are of equal moment. Fortunately for agriculture the deposits of phosphates and potash will meet our needs for centuries and nitrogen we can fix from the air. But again the reserves of phosphates and potash are most unequally divided, marking the dependence of one country on another.

Reserves of metals may present the world before long with a much more difficult problem, There will be no lack of iron, aluminium or magnesium, although with lower grades of ore the cost may rise. Estimated reserves of copper, zinc, lead and tin are adequate only for some decades, and if all the world used them at the same rate as the United States, there might be shortages within ten years. Geologists are not optimistic of finding large new ore fields and so we shall be faced with an iron/light alloy metal economy, reserving certain non-ferrous metals for the uses for which they are essential. Here again the accidental distribution of ore deposits reveals the interdependence of countries in an acute form.

The global sufficiency of energy reserves is but a poor consolation for the countries that have few. The recent State Department Survey shows that half the world's visible reserves lie in the United States or the U.S.S.R., and another third in nine other countries. Of the remainder none can claim as much as one per cent., so that their long term development may call for new techniques. These inequalities are reflected in consumption to-day. At one end of the scale the United States use yearly the equivalent of 9 tons of coal per head,while at the other India and China are using less than one-fifth of a ton per head. The figure for Britain is a little over 4 tons of which one-third provides domestic needs of heat, warmth and food, leaving nearly 3 tons for transport and industry.

What part is energy to play in the problems the world is facing now—the destruction and disruption of war, the economic unbalance of the older countries, and the claims of the less-developed countries with their ill-nourished millions for a better share in the world's dividends? Energy is essential to increased production both in the factory and on the farm. The needs of the more backward countries are transport, fertilisers, irrigation, heat and power. Transport to diversify their social and economic pattern, fertilisers to sustain and increase fertility, irrigation to bring the water on which all life depends, and heat and power to develop their indigenous resources.

The Need for Greater Efficiency

All this means fresh demands. The nineteenth century was wasteful of its easily won materials, but now, thanks to the continuous improvements in technique, the world's demands for heat and power are being met largely by improved efficiency. In Britain until 1938 the total fuel consumption was almost unchanged for a quarter of a century. During that time the population had increased by 10 per cent. The use of heat and power had in industry been extended to secure increased production from human effort. New industries had arisen and the standards of domestic comfort had increased. The striking fact emerges that the additional energy required with the progress

in those years came from the same amount of fuel by increased efficiency of use. But as the wartime economies proved there were still great improvements to be made and that is still true to-day.

What are the prospects of meeting future demands by improved efficiency of existing practice, by the transport of energy or by new methods and new sources? To simplify the problem I will discriminate between the four energy industries—coal, electricity, gas and oil—and the uses of the products they provide. It is in the energy industries that the major advances have been made. The most modern types of plant in each have reached a point where only fractional progress is likely to be made, and where the limits may be the discovery of new alloys or the higher cost which the gain in efficiency would not justify. There are still large savings to be made by raising the standard of all plant to that of the best practice. The generation of electricity has a special problem of its own, the loss in the cooling water of the condensers of about 60 per cent. of the heat employed. Where steam is needed for any purpose in addition to power this loss can be avoided by using steam from the low-pressure side of the turbine instead of condensing it. In industries using steam, the generation of electricity before the steam is used for heating purposes would secure large fuel economies. The other plan of using low-grade heat for district heating involves heavy capital costs if it is introduced in built-up areas, unless hot-water heating systems are already installed, but it would be more attractive in new development schemes with a sufficient density of population. With the continuous increase in the demand for electric power and the heavy capital cost of generating equipment the problem of the combined use of heat and power is of major importance.

Turning now from the energy industries to the use of heat and power in industry, in transport and the home, it is there that inefficiencies of one kind or another combine to make the overall efficiency of use in Britain probably less than 15 per cent. Here is the urgent need for large economies to save the coal that will be needed for increased production. The future of our industrial efficiency will be closely linked with our efficient use of coal, the main raw material on which our prosperity depends.

To-day's Developments

Before looking to the future let me touch on a few developments that lie immediately ahead. The gas turbine, a new prime-mover, has proved itself for aircraft use both as turbo-propeller and turbo-jet. Several land and marine types are now being developed to use gaseous, liquid or solid fuels. It has the advantage of compactness, it can be used where no cooling water is available, and it is specially suitable for burning by-product or waste gases. Underground gasification of coal is being tried in several countries and if successful the gas turbine may possibly be the best means of utilising the gas obtained. In Britain much depends on the success of the gas turbine using coal by one means or another.

Kelvin's heat pump, the refrigerator in reverse, with its high efficiency of power in supplying domestic heat is now on trial. Its economy is greatest in latitudes where its use for air-conditioning in summer balances its use for heating in the winter. In industry too the principle of the heat pump is being applied with great economy to recover the heat lost in evaporation.

In Italy the internal heat of the earth has been used successfully in a geothermal power station on an ever increasing scale for more than thirty years. Difficulties and dangers in drawing steam from great depths and using it have all been overcome. Italian engineers and scientists have set a great example that could be followed wherever thermal springs or volcanic steam are found.

Few advances have been made in using wind power, one of the earliest forms of energy used by man, but now with the progress in aerodynamics, new experiments are being tried which may produce power economically to be fed into a grid system and thus save coal. At favourable sites in Britain it is expected that wind-driven generators weighing about 300 lb. per kW. will produce 4,000 kWh. per kW. per annum.

Future Possibilities

What new means of tapping Nature's stores of energy are likely to emerge and

bring it where it's needed? The unequal distribution of energy sources will make its transport of increasing importance in the future, and here the possibility of transmitting electric power over long distances by high-voltage direct current has passed the experimental stage and is waiting for development. By this means it should be possible to transmit large blocks of energy economically for perhaps a thousand miles by underground or submarine cable and thus link consuming centres with new and distant sources of hydro-electric power. There is too the interesting possibility of linking the British and European Grids and thus securing a better balance between capacity and requirements by taking advantage of the seasonal variations in capacity and the diversity of demand. Such a scheme would of course be complementary to the distribution of power by alternating current, and it offers an opportunity for us to share in European co-operation without raising the delicate problems of supra-national authority. A link of 250,000 kW. capacity should be of mutual service to both sides by providing the equivalent of a large modern generating station as stand-by plant.

The tides in favoured spots are obvious sources of energy and if more courage had been shown in pre-war years in carrying out schemes like the Severn Barrage, they might to-day be economical and saving fuel.

Then it is tantalising to think of the immense amount of energy reaching the earth by solar radiation and the small use we make of it. The energy reaching the earth's surface is some 50,000 times as great as our consumption. Forty per cent. of it is used in evaporating water and one per cent. in lifting it into the atmosphere, whence we derive our water power. Most of the rest is absorbed and lost by radiation in the night. One per cent. of the radiation falling on vegetation is used in photosynthesis, whence we derive our food and fuel. Many experiments have been tried to make more use of solar radiation—its concentration by mirrors to give high temperatures, its storage in hot water or melted salts and the generation of electricity by the photo-electric cell or thermo-couple. None have so far produced an economic solution, but sunshine is so often available at places remote from other energy sources, that we surely have a duty to posterity to pursue long term researches in this field while we are living on reserves.

One large-scale experiment in using solar radiation is being made by the French Government on the West Coast of Africa. A turbine will be driven by steam obtained by evaporating under vacuum the warm surface water of the sea at 28° C., the condenser being cooled by water at 8° C. drawn from a depth of 400 metres. Another interesting possibility in Egypt is to drive turbo-generators in the Qattara Depression by water drawn from the Mediterranean 45 miles away, relying on solar radiation to evaporate the water from the tail-race as it accumulates in the Depression.

Last year Sir John Russell spoke of the possibility of raising the efficiency of plant photosynthesis to produce more food in a given area. Experiments have shown that this is possible with certain algae, but the land-grown plant supplies the structure of its own factory and draws its own raw materials from the air and ground. The use of algae would be an industrial process and the land-grown plant may prove to be the more economical in operating costs. However, the work on algae is in its infancy and it would be premature to speculate about its economy.

Mayer ended his short prophetic paper in 1842 by saying that the inefficiency of the steam engine justified the attempt to produce 'motion' by other means than the sacrifice of carbon and oxygen, for instance by electricity obtained by chemical means. An economical fuel cell in which the free energy of the oxidation of carbon is converted into electricity has so far evaded discovery, but it remains one of the long-distance goals of research.

Nuclear Energy

The story of nuclear energy shows what can be done if science is used lavishly with all the resources of the modern world to penetrate Nature's secrets. Now having wrested Nature's secret from her, comes the task of using her great store-house for the good of man. I need not dwell on the advantages of that concentrated source of

energy, with a potential several million times that of coal, if it could be tamed. Energy could be taken to remote places of the earth when it is needed. Deserts lacking only water might be made a paradise. But to-day the unsolved problems of these peaceful uses loom so large. Once more we see how hard it is to be creative compared with the ease with which man can destroy.

It was clear from the discussion at the Fourth World Power Conference in London that the difficulties to be overcome before the use of nuclear energy becomes economical are most formidable and their solution will require intense effort over a long period. New engineering materials must be found with properties never before investigated or required. They must be stable over long periods under intense neutron bombardment and must not cause loss of neutrons by absorption. The removal and disposal of the radioactive products from the reactors will be a costly and puzzling task. With the world's need of energy, it would be a tragedy if the fear of competition from nuclear piles were to delay in any way the immediate development of the more conventional and better understood sources of energy. It may well be that the first use of fissile elements will be to transport energy to places far remote from other sources. Meanwhile the nuclear plants are yielding good dividends in the radio-active isotopes they produce. These provide a most powerful new technique of research, which has given us both a new understanding of many of Nature's processes and new methods of production control.

Auto-Mechanisms

The by-products and radiations of these nuclear plants are so dangerous to life that their operation is only made possible by automatic controls. This brings me to the latest chapter in man's use of energy, the new science of auto-mechanisms, which can replace the human senses and control machines. These modern robots may seem to bring us nearer to the fear of Erewhon of the mastery of the machine, but just as the machine was the saver of manual labour, they are substitutes for the drudgery of the human brain. To-day the energy of the electron is harnessed by photo cells, by radar pulse and other means through valves and amplifiers to watch and act. Usually these devices are more accurate than the human senses and their action in micro-seconds eliminates the slow reaction time of man. They leave an automatic record too of what they have done. But in claiming the superiority of these man-made mechanisms for their specific purpose over man himself, I must make one exception. No auto-mechanism has I think approached the delicate perception of the sensory organ with which my brother chemists are so well endowed—'Their noses infinitely wise. Their minds being memories of smells.'

In any task that they are set to do, these controls must be fed with information to tell them when to act and the result. For instance in drawing wire through a die at constant tension, the auto-mechanism must detect a change of tension and at once adjust the speed of reeling-up until it knows the standard tension has been reached again : just as the senses tell the muscles how far their task has been performed. There are even electronic mechanisms to simulate the performance of their own kind and say how well, or ill they do the work for which they are designed.

Sometimes the task is not so simple and calculations are involved, and these the electronic computer can do much faster than the human brain. And there are calculating machines with electronic memories, which can be set to make most complex calculations which would be impossible by human means owing to the time involved. These electronic computers can perform set tasks without ability to think. Errors may occur if a vacuum tube burns out or a switch fails to act, and now a new device enables them to detect such errors, locate them and correct their own mistakes.

This latest venture in the use of energy must have most far-reaching effects. These automatic controls and precision techniques, consuming almost negligible quantities of current, will save man-power and add greatly to the economy of production, thus reducing the quantity of energy required. In the future one of the indexes of economic progress should be, not the energy used per worker, but the

output of goods and services per horse-power employed.

Energy and the Future

Now, as I near the end of my address, I wonder, have I claimed too much for progress—progress, that missing word among the Greeks? Have we replaced the Golden Ages by an Age of Steel? Has man's use of energy brought happiness or not? There is no yardstick that can tell us, in Priestley's phrase, whether the 'greatest happiness of the greatest number' has increased. We see the contrast of man's swift advance in understanding Nature and the troubles of the world to-day. Is the myth of Daedalus for ever true? Sir Arthur Keith ends his 'New Theory of Human Evolution' with the words 'In the clash and turmoil of this modern world we are hearing the creaking wheels of the machinery of evolution.' Is it inevitable, as he concludes, that in this great revolution through which we are passing the old primitive enmity of nations, the urge to self sufficiency will grow stronger? What ground is there for hope?

Once more I turn to Carnot's essay for the answer. In 1824 he wrote 'Navigation due to the steamship has in a measure drawn together the most distant nations. It tends to unite the peoples of the earth as if they lived in the same country.' His prescient eye had seen that energy would bring the world together. We see to-day the stirring of a new world consciousness that must in time bear fruit, a new awareness of the load the modern Atlas has to bear, the problems we must face: the growing strain of increasing population, the malnutrition and endemic sickness of perhaps half the world, the inequalities between the more forward and the backward peoples, the gradual depletion of resources and their unequal distribution, and, by no means least, the human problem of changing the way of life and outlook of many millions. The hope lies in man's new understanding of Nature's processes, in his more efficient use of her resources and in the growing recognition of the dependence of one nation on another. Wendell Wilkie saw this so clearly in his classic phrase 'to raise the standard of living of one man anywhere in the world is to raise the standard of living by some degree of every man everywhere in the world.'

These problems are the challenge to the science and engineering of our time. Only they can solve them—if allowed, and if men's minds are bent on quest of plenty not on quest of power. That is the issue for the world to-day, a world so divided for the moment. The orderly solution of these problems must depend on a knowledge of conditions and the needs of each country, on a survey of its natural resources, its human geography, its economic structure and its capacity to produce and consume. The limiting factors will then appear and the problems they present can be attacked in their priority. The overriding importance of such surveys is slowly being recognised and the technique of making them is under way. Much material of the kind has already been collected by individual countries, by industries and by the agencies of the United Nations. Eight years ago I wrote 'Unless all this information is brought together to a central point—a United Nations Economic Bureau—it will remain uncoordinated, with unknown gaps, and there will be no comprehensive picture which may serve as a basis for the processes of reconstruction which will merge gradually into the processes of international trade.' That is still true to-day and last September at Lake Success many speakers emphasised the need for such inventories of world resources so that each country might know the resources and potentialities of the rest. Only in this way can the natural wealth of the world, distributed in such an accidental way, best be used to meet its ever growing demands.

In all these plans energy will play a vital part. So often its availability will dictate the pattern of development. It may well be the limit in Bacon's words 'to the effecting of all things possible.' Throughout the ages it has been the key to progress and it has not lost its power. On man's wise use of energy depends so much the future of this troubled world.

MATHEMATICAL NOTATION

Address by

PROF. E. H. NEVILLE

PRESIDENT OF SECTION A

YOU are all mathematicians, but almost all of you are something more than mathematicians ; you are astronomers, meteorologists, physicists, or whatever it may be. Few mathematicians who are nothing but mathematicians attend the meetings of our Association regularly, and in this Chair I am not only a fish out of water, but a very small fish at that : you should see the ones that got away ! An enquiry into the reasons for this state of affairs might lead to interesting observations on the purpose of conferences in general and of our own meetings in particular, as well as on the differences between one subject and another in respect of semi-vulgarisation, but it would be in the worst of taste to offer you who are here nothing but a disquisition making excuses for those who are not. Let us agree with the French that being absent they are in the wrong, and leave it at that.

Nor will I repeat, in his words or my own, the analysis which Hardy gave at Hull in 1922 of the peculiar difficulties which confront a pure mathematician who has the duties of this Chair to discharge. I could I suppose fill an hour with anecdotes, or with platitudinous talk on the distinctions between pure mathematics and applied mathematics, but I am not a social historian or a philosopher, and I should do better to forgo the privilege of addressing you than to use your time frivolously.

A comprehensive survey of the subject of mathematical notation would begin with prehistoric legends about primitive number-signs, and would explore the South Sea Islands for unrecorded stories of the same kind. Scales of notation and the strange failure of the Mediterranean peoples to discover the principle of place-value would lead to digressions on the abacus and on Archimedes' sand-reckoner, and to speculations on methods of computation with Roman numerals. Towards the end of an hour we might see the shadow of the coming algebra. The first instalment of a survey undertaken on this scale could have little bearing on the advancement of science in our own day, and I do not propose to take you back, except in casual reference, more than a century and a half. An eighteenth-century mathematician opening one of our mathematical periodicals would find many pages that he would read quite easily when he had been told the definitions of the special functions with which they were concerned ; he would seldom be able to follow the proofs, since they would generally be quoting theorems established since his time, but he would understand the assertions made. But other pages would remain completely unintelligible, in a profounder sense ; he would feel, not that he did not know what the symbols stood for, but that he could not see how they were being used. It is to these developments of symbolism that we are to give briefly our attention.

The great contribution of the nineteenth century to mathematical notation was the acceptance, gradual but in the end complete, of the non-numerical variable. In the middle of the century there flourished a powerful calculus of operations of which very few traces are to be found in modern mathematics. No sooner were interpretations found for addition and multiplication than operators were detached completely from their victims, to be resolved into partial fractions, expanded in Taylor series, and generally treated with the most reckless disrespect. In consequence, mathematicians of that time could solve problems beyond the power not merely of their predecessors at the beginning of the century but of their successors at the end of it.

The Weierstrassian precision and logic branded as piratical all that was adventurous in symbolical mathematics. De Morgan fretted under the restraints in which his superb critical faculty compelled him to acquiesce, and only Heaviside had the confidence to carry on under the Jolly Roger. Heaviside's attitude was much that of the pure mathematician of to-day to the expert in logistics ; if criticism of the theory of the real variable leads to the conclusion that the whole of mathematics beyond the elementary arithmetic of integers is meaningless, so much the worse for the logical foundations of the criticism.

Heaviside, I need hardly say, has been proved right, and the Cambridge school for which he demanded justice has taken no small share in the demonstration. This is not to say that the operational calculus which couples Heaviside's name with that of Laplace is the well-founded substitute for the old calculus of operations. The operational calculus, with its Fourier transforms and Mellin transforms and Laplace transforms, deals with correlations that Cauchy would have understood quite easily. Notation can hardly be said to be established firmly, and it offers some features of interest, but none on which there is time to dwell.

The uncritical processes of the calculus of operations are still employed in the calculus of finite differences. It is not that this calculus is less careful of logic than other branches of mathematics, but that the formulae required are almost always in the ultimate analysis nothing but transformations of polynomials. The use of meromorphic or transcendental functions of the operators is only the most efficient technique for discovering the polynomial identities that are wanted, and no interpretation except that of polynomials is essential to the argument, whatever the intermediate steps may seem to imply.

As far as it deals with operators characterised by properties, not with particular operators, the calculus of operations survives in the theory of groups and in our many algebras. The importance of the calculus was not in establishing theorems, but in establishing a frame of mind in which the non-numerical magnitude was received without apology in mathematical analysis.

The corollary that the rules of calculation were to be discovered from the properties of the variables and could not be postulated was one that mathematicians were slow to draw. The naive view was that it was only if the elementary properties of the variables implied formal rules identical with those of elementary algebra that the subject was one to which mathematics could be applied. Whitehead assigns to Boole the distinction of being the first to recognise clearly that if the formal rules were different, it was a new branch of mathematics that was being created but none the less it was mathematics.

Evidence of the belief that the variable in mathematical analysis must be in some sense numerical survives in the name of the complex number. The difficulty with the complex number was in interpretation, not in notation. The equation

$$x^2 - 12x + 325 = 0$$

is surely as well entitled to its x as the equation $x^2 - 12x - 325 = 0$, even if it turns out that x must have one of the unreal values $6 + 17\sqrt{-1}$, $6 - 17\sqrt{-1}$ instead of one of the well-understood values $25, -13$. The x that is $6 + 17\sqrt{-1}$ has a real part 6 and an imaginary part $17\sqrt{-1}$, but what is it, itself? We know so well a variety of different satisfactory answers to this question that we are unable to see the difficulty of it. There is, oddly enough, ground between Sections H and J of this Association which is unclaimed by Section I, and we must appeal to the anthropological psychologists who till that ground. They tell us how the mind of primitive man was working when he began to count, perhaps 150,000 years ago ; surely they can tell us how the mind of a mathematician worked a mere 150 years ago ! We nowadays take the next step easily. The x, whatever it is, is identified completely by the real part 6 and the coefficient 17 in the imaginary part ; its specification is bound to consist of the two real numbers 6, 17 each playing its allotted part. Why embark on a metaphysical search for something to possess the specification, when the specification itself, about which there is no mystery, is all we can ever need ? Identify the number with the specification itself, and no problem will remain.

There must have been some reason why this solution was not obvious from the first. It is always hard to appreciate the extent to which familiarity with the conclusion of an argument bludgeons the commonsense that has made the argument necessary. There is much more to be said for the geoplanarians—or geoplatylogists, for De Morgan was a better classical scholar than Kipling—than we are ready to admit. In the case under discussion, I can only conjecture that if you are looking for *a* number, the one thing that will not satisfy you is a pair of numbers. This may seem a quibble, but there is evidence of the same dissatisfaction in early writings on matrices and determinants. The first matrices to be studied were inevitably square, and in much of the pioneer work on determinants it is difficult to say whether the name and symbol are being used for the matrix or for the value. The feeling that every matrix ' ought ' to have a value persisted to its climax in Cullis' massive but barren theory of determinoids. From one point of view, the determinant of order n was a particular multilinear function of n^2 independent variables. The matrix was only a convenient arrangement of the variables, and without a function to evaluate, a table of arguments is peculiarly empty. We find Cayley speaking sometimes of ' the matrix considered as a single quantity,' but it was not till the matrix was presented as an operator transforming one set of variables into another that it recovered the individuality which made it easy to denote a matrix by a single letter and to undertake the development of an algebra of matrices. It was this same individuality which was conferred on the complex number by Argand and Wessel to end the scandal of the square root of minus one, the imaginary unit for which mathematicians had put up only a pragmatical defence.

To be frank, the whole history of mathematics is fit gossip for a school for scandal. This is notorious in geometry ; no-one ever pretended that Euclid succeeded in defining a straight line. It is equally true in the other great division of the subject. You remember Branch Cabell's small son, in whom a governess was endeavouring to implant the conviction that two and two make four. ' The child stayed sceptical. He was reservedly polite about a rational " Suppose you had two apples, and I gave you two more apples, how many apples would you have then ? " He conceded with readiness, not unflavored with resignation to the obtuseness of grown-up persons, that in such circumstances he would have four apples. . . . Yet that two and two, in consequence, make four, he excluded as a logical inference : and he depreciated that inference by stating it did not mean anything. He was, of course, quite right. For that " two and two make four " becomes, the very instant that you pay this familiar axiom the childish trick of thinking about it, at best an unprovable hypothesis. That two apples and two more apples compose four apples is . . . plain enough. Or, you may change your unit to a penny, a match, a pencil, or to a bungalow, and still produce convincing evidence to prove your arithmetic. But the mathematician requests us to consider an abstract " two," to believe in two apples with the pomaceousness removed : his incorporal and incorporeal " two " has never existed and never can exist. His " two " is not merely a fiction, but an inconceivable fiction.'

When, up to date and sophisticated, we ask whether, history notwithstanding, we *are* under obligation to attach meanings to our symbols, we are answered in two of the best known epigrams in mathematical literature. ' A symbol which has not been properly defined is not a symbol at all. It is merely a blot of ink . . . which has an easily recognised shape. Nothing can be proved by a succession of blots, except the existence of a bad pen or a careless writer.' This is Whitehead's answer, repeating in general terms a protest of Chrystal's twenty years earlier : ' Some expounders of the theory of the exponential function of an imaginary argument seem even to have forgotten the obvious truism that one can prove no property of a function which has not been defined.' Russell's answer is very different : ' Mathematics may be defined as the subject in which we never know what we are talking about, or whether what we are saying is true. People who have been puzzled by the beginnings of mathematics will, I hope, find comfort in this definition, and will probably agree that it is accurate.' We do not expect to

take language of this quality literally, but it is somewhat disconcerting to find the two authors in flat contradiction. In our perplexity we are consoled to observe that each author had in fact achieved his great reputation by practising the doctrine which the other put into words.

Whitehead, not yet a philosopher at Cambridge, Mass., but a mathematician at Cambridge, Eng., was the author of two tracts on the axioms of geometry (half a crown each, for 72 and 82 pages—no wonder we formed book-buying habits) and of *A Treatise on Universal Algebra*, vol. I. Few people, I suppose, open Whitehead's magnificent first book now, but no-one can read it without longing vainly for the abandoned sequel, and speculating on the probability that if Whitehead had been faithful to his first love, serious algebra in Cambridge, Eng., would not have had to pass through a phase of being nothing more than the groundwork of algebraic geometry. This is a digression from a digression ; what I stepped aside to say was that in *Universal Algebra* Whitehead introduces for the operation to be called multiplication a new symbol, precisely in order to make it perfectly clear that when he talks of addition and multiplication he is as innocent as Russell would like him to be of knowing what he is talking about.

Russell's *Principles of Mathematics* was another solitary vol. I, for a fresh start was made with *Principia Mathematica*, of which it was only a projected fourth volume that succumbed to the Buonarottist element in Whitehead's character. Russell's triumph in the *Principles* was to give what to the logic of the time were precise definitions of natural numbers and real numbers. Until the appearance of that book, everyone outside the handful of readers of Frege's *Grundgesetze der Arithmetik* was content to use numbers of all sorts without worrying overmuch. The mathematician can, as Kronecker suggested, disclaim responsibility for the natural numbers, and his interest in a definition of them is only that of the man in the street. But the real numbers are his own invention, and he ought to be able to say what he has done. I well remember struggling as a schoolboy, and of course struggling helplessly, with Chrystal's account of the rational onefold, and then getting hold of Serret's *Algèbre Supérieure* in expectation of finding there an explanation better suited to my age. How wrong I was ! Many years later I found Dedekind, but still the real number seemed only to be associated elusively in some undefined way with the mental act of specifying a partition of rationals. In the words of Beman's translation, we create a number which we regard as completely defined by a cut ; we say that the number corresponds to the cut, or that one produces the other. What a relief it was to turn from this mystical creation of an object still indescribable, or at least not described, to Russell's straightforward identification of the real number with a class of rationals. We had not known what we were talking about, but now our symbols were no longer blots on paper.

So Whitehead and Russell between them supply the grains of salt to be taken with their two epigrams, and the mathematician is left with the pleasant conviction that if he doesn't make mathematical mistakes he needn't lose his mathematical reputation.

Much light is thrown on the acceptability of symbols by the evolution of the concept of the quaternion. No other mathematical genius of the first rank has left as detailed a record of gropings towards a great discovery as Hamilton, who, but for his infuriating typographical modulations of emphasis, is easy to read. Hamilton reached the quaternion after a long and abortive attempt to construct an algebra of triplets, consistent in itself and applicable to geometry, and what I find significant this morning is that writing in the preface to his *Lectures on Quaternions* ten years after the discovery, he never once uses an abbreviation for a triplet. The conclusion is irresistible : it is because he thinks of the quaternion as an operator which converts one vector into another that it has for him the individuality demanded by his sense of propriety in notation.

That the quaternion, in the form $w + ix + jy + kz$, was the variable in an algebra of sets, Hamilton was well aware. Cayley included quaternions as a special case in a long essay on multiple algebra which was an expansion of a few paragraphs in his Presidential Address to this Association in 1883. Cayley was not

concerned with the operational character of the quaternion, and he knew that he was dealing with the set of constituents (w, x, y, z). Nevertheless, it is not till he has introduced the four units $1, i, j, k$ and written the set as $w + ix + jy + kz$ that he uses a single symbol Q, and if we are to credit him with thinking of Q merely as the set and not as some 'generalised number' which 'has' the four constituents we must overlook his choice of 'extraordinaries' as the name for the units.

The notions of operator and group of operations established the non-numerical variable firmly in analysis, and since a matrix of any shape could be interpreted as the specification of an operation, the matrix needed no special justification. There was of course an element of self-deception in the mathematician's solution of his difficulties. Commonsense could not survive five minutes' philosophical inquisition on the nature of the concept which seemed so satisfactory, and it is more than likely that commonsense would soon be defending a mere specification, without insisting that there was in fact any such thing as an operation to be specified. I am not propounding a philosophy, shallow or deep; I am only suggesting that it was in the presence of non-numerical variables that the notation of analysis differed most in character at the end of the nineteenth century from what it had been at the beginning, and that it was as symbols for operators that these variables forced their entry.

Before dealing with the specific contributions of the last half century to mathematical notation, I propose to say a little about the problems of the subject. Strictly speaking, notation cannot be right or wrong, but only good or bad; what is the good word for the mathematician whose notation is bad, I don't know.

The most irritating of notational misfortunes are those where we should all like a slight modification in a usage that has become classical. No-one can doubt that if we had a free hand to-day we should have a symbol for the radial measure of one right angle, not of two; the economy would be incalculable. We cannot say that the judgment of William Jones was gravely at fault. One right angle or two, what did it matter? Or that Euler was nodding when he adopted Jones' suggestion. Should he have foreseen Fourier's theorem? No-one was to blame. Nevertheless it is almost certainly too late to make a change. The one thing we must not do is to use π with a new meaning. An international agreement to replace $\frac{1}{2}\pi$ by $\tilde{\omega}$ in future might effect the reform, but it would be many years before the benefits of the new system would repay mathematicians for the awkwardness of the transitional period.

A similar case, which affects only a few of us and in which we can't help thinking that the pioneers should have known better, is that of the covariants of a pair of conics. Of course the discriminant of $\lambda S' + \mu S''$ should be written with binomial coefficients, but is it worth while to do anything about it? And the canonical cubic in Weierstrass' elliptic function,—what was Weierstrass thinking of to write $4w^3 - g_2w - g_3$, not $4(w^3 + 3g_2w - g_3)$? Our formulae are cluttered up with powers of 4 and 12, and some of the signs look unnatural. Dare we write

$$4(w^3 + 3h_2w - h_3),$$

and leave the future to choose between g's and h's?

In these examples, we do know what we should like. The case of the gamma function is more complicated. Euler studied the integral from 0 to 1 of $\log(1/x)$ to the rational power $p/q - 1$, to find for what values of p and q he could evaluate the integral in finite terms; he denoted the integral by the fraction p/q between square brackets, and he found a number of reduction formulae. It was Legendre who replaced Euler's fraction by a continuous variable a and regarded the integral as defining a new function of this variable; Legendre denoted the function by Γa. Euler's investigations gave exact values for the function for various rational values of the argument, and his reduction formulae became properties of the function. In particular, the relation $\Gamma(a + 1) = a\Gamma(a)$ reduced evaluation in general to evaluation over the range from 1 to 2, and with characteristic thoroughness Legendre gave a numerical table of $\log \Gamma a$ for this range at interval 0·001 to 12 places of decimals with third differences; this table formed part of the second volume of Legendre's *Exercices de Calcul Intégral*, 1817, and was

reprinted nine years later in the second volume of the masterpiece of which we sometimes forget that the full title is *Traité des Fonctions Elliptiques et des Intégrales Eulériennes, avec des Tables pour en faciliter le calcul numérique*. Presently Gauss came by a different route to a function which was in fact the same one, except that he was writing Πx where Legendre had written $\Gamma(1+x)$. Formal analysis is permeated so thoroughly with the gamma function that it would be rash to assert that there is not one formula in which the argument looks happiest in the form derived from Euler, but there is no doubt whatever on which side the balance of advantage is to be found. The modern theory of interpolation, in which Euler would have revelled, clinches the matter. When x is a positive whole number n, the value of Πx is $n!$; in other words, Πx is one function of the continuous variable x obtainable by interpolation from the function $n!$ of the integer n. This property cannot be sufficient to identify Πx, but Nörlund has shown convincingly that there is one process of interpolation from n to x so natural that we must accept it as fundamental, and it is Πx that is produced from $n!$ by this process. And now we see what a handicap an unlucky slip in notation can impose. The use of Σu_n for the finite sum $u_1 + u_2 + \ldots + u_n$ and of Πu_n for the finite product $u_1 u_2 \ldots u_n$ is common and harmless in elementary work, and in this notation Πn does mean $n!$, so what more natural than to use Πx for the associated function of x ? But we have made Πu_n ambiguous whenever u_n is not n. It is true that this ambiguity is latent in the primitive use of Π when u_n is itself a whole number, and that then a convention is necessary to remind us that Πu_n is not to mean $u_n!$ This is only a rare possibility, and in practice we shall never go wrong. It will be quite different if as often as not we do want Πu_n to mean the value of Πx when x is u_n and not the product of n factors. The attempt to associate the function of x too closely with the general symbol for a product was bound to fail, and a special symbol must be adopted. Well, if we can't use Gauss' notation we may as well go back to Legendre's for there is no fundamental distinction between the functions. And this of course is where mathematicians made their mistake. The torture of a dripping tap is very real, and the extra 1 in the gamma function is, as Jeffreys puts it, a minor but continual nuisance. The last twenty years have seen a determined effort to return to Gauss' argument for the function. The lead was given by the Tables Committee of this Association in its first volume, and has been followed by Jahnke and Emde in their *Funktionentafeln* and by the Jeffreys in *Mathematical Physics*. Notation is still a problem. Everyone can agree to call the function the factorial function, but it must be admitted that the proposal to transplant to analysis the symbol familiar in elementary algebra for the arithmetical function $n!$ involves difficulties that may yet become acute. Alone among functional symbols ! follows its argument, with the result that such simple combinations as $\log z!$ and $z(z-1)!$ introduce a new type of ambiguity into our notation. Moreover, we are deprived of the ordinary use of a dash as a symbol of differentiation : $z!'$ is unlikely to find favour. This matters less because it is not the derivative but the logarithmic derivative of the factorial function that comes into analysis, and when $\Gamma(1+z)$ is replaced by $z!$ some change in $\Psi(1+z)$ follows as a matter of course. We might have expected another sign of punctuation to be pressed into service, but paradoxically, provocatively, pusillanimously, or perhaps piously, digamma has been chosen, and $z\digamma$ is still available for any mathematician who has a function of questionable importance to investigate.

Before quitting the factorial function let me remark, in case someone feels that to write $(x+iy)!$ is an enterprising thing to do, that as long ago as 1885 Forsyth, temperamentally a stickler for etiquette, wrote the formal solution of the differential equation

$$x\frac{d}{dx}\left(x\frac{d}{dx}-1\right)\left(x\frac{d}{dx}-2\right)\ldots\left(x\frac{d}{dx}-\overline{n-1}\right)y=0$$

as

$$y=\frac{(\mathfrak{I}-n)!}{\mathfrak{I}!}0.$$

Have you noticed how serious is the lack in the theory of functions of an operational

notation for the logarithmic derivative? The name itself we can accept as conventional, although it leaves something to be desired: in the real field a real function is described in terms of one which may have to be complex, and in the complex field a single-valued function is described in terms of one which is many-valued. But there is a multitude of theorems in which the function immediately involved has the form of a quotient $f'(z)/f(z)$, and both the general theory of functions and the investigation of special functions would benefit enormously from a standard notation in which the functional symbol f and the argument z made only one appearance. In the cases of the gamma function and the various theta functions, including the Weierstrassian sigma function, the logarithmic derivative has an independent symbol, and in the cases of the circular functions and the Jacobian elliptic functions the logarithmic derivative is expressible in terms of the set of functions concerned, but the need for the notation in the general theory is not the less acute because in a few cases special provision is made.

If we had a special symbol, we should probably never write $f'(z)/f(z)$ in full, and that might not be an unmitigated advantage. We often spoil the pattern of a determinant because we cannot refrain from contracting $a_1b_1 + b_1a_1$ to $2a_1b_1$, and in turning over the pages of an old book I often think how stupid we are not to be willing any longer to write axx instead of ax^2 as the argument of an exponential and so to avoid without any difficulty the typographical problems of a three-storey symbol.

Superficially it would seem that if one magnitude is the square of another, or one function the negative of another, distinct symbols must be not merely redundant but indefensible. Nothing could be further from the truth, and a mathematical sense of proportion is often exhibited in the play made with alternatives. Sometimes it is even worth while to retain two symbols which are completely equivalent.

In the classical theory of elliptic functions a set of functions was determined by a modulus k which was a real number between 0 and 1, and the set had a real quarterperiod K and an imaginary quarterperiod iK'. We now begin with a pair of quarterperiods, which in general are both complex numbers, and we infer the value of the modulus, also as a rule complex. If we use K_c instead of K and K_n instead of iK', we not only avoid a suggestion which would be misleading but we find that we relieve the memory of an intolerable burden, since K_c is a halfperiod of the function sc$u$ and K_n is a halfperiod of the function sn$u$. The sum $K + iK'$ is a halfperiod of sd$u$; the classical theory has no symbol for this sum, but we denote its negative by K_d, thus securing between the three halfperiods the symmetrical relation $K_c + K_n + K_d = 0$. Each of the three magnitudes K_c, K_n, K_d is a halfperiod of the function with which its symbol shares the distinguishing letter, and is a quarterperiod of each of the other two functions. In the plane of the complex variable u, each of the points K_c, K_n, K_d is a simple pole of the function with which it is literally associated and is an ordinary point of each of the others. Also each of the functions has a simple zero at the origin. It follows that cn$u$, which is the quotient of sn$u$ by sc$u$, has a simple zero at K_c and a simple pole at K_n, and that K_c and K_n are quarterperiods, but neither of them is a halfperiod, of cn$u$.

No-one denies the advantages of this expressive notation, even for a system which happens to be classical. The point I wish to emphasise is that no-one wants K and K' to disappear altogether. The most valuable applications of elliptic functions at the present time are undoubtedly those that use conformal transformations in which the system is classical and the variable is complex. Dissection into real and imaginary parts is the essence of these transformations, and it would be nothing but a nuisance to have the real character of K_c and the imaginary character of K_n obscured by a mistaken adherence to the noncommittal notation. What we do is to bring K and K' back by defining K as K_c and K' as K_n/i; formulae then take the classical form and are ready for any of the applications in which a classical system is wanted. We cannot have the best of both worlds unless we can speak two languages.

In the same field of Jacobian functions

I find another warning against too great eagerness to label a symbol as redundant. The details of a Jacobian system depend on the values of the two moduli k, k', but the functions in the system can be defined, for example by inversion of integrals or by triads of differential equations, in ways which involve only the square of the modulus k. There is no paradox : the set of functions is determined by certain patterns of poles and zeros, but some of the constants in the formulae depend on the way in which these patterns have been labelled. We cannot properly understand the dependence of the system on the one constant which determines it unless we introduce a symbol for that constant, but if we write k^2 as c for that purpose, our formulae will lose precision if we leave k and k' expressed always as irrational functions of c.

It is to elementary metrical geometry that I should turn for the simplest example in which two numbers of which one is only the negative of the other have balanced claims to priority. But time is short, and I must concentrate on one example, the cause of the bitterest and stupidest quarrel in mathematical history. It was only because Hamilton found the scalar part of the product of two vectors to be $-rs\cos\varepsilon$ that Heaviside was able to say, with an air of patronage that so great a man had no need to adopt, that quaternions were doubtless of immense value in the study of quaternions. There is no real room for dispute. The surveyor wants cosines as much as the engineer wants work. But Hamilton was not asking what were the useful functions of two vectors, he was asking what were the rules of combination of specified operations on vectors. He had found that a quaternion was expressible as the sum of a scalar part and a vector part ; a vector was therefore a degenerate quaternion whose scalar part was zero, and unless the whole of his calculus was to be inconsistent, the product **ab** of two vectors must be formally the same as the product of the degenerate quaternion $0 + \mathbf{a}$ into the degenerate quaternion $0 + \mathbf{b}$. Hamilton at this stage of his investigations had no choice whatever, and one thing he was emphatically not doing was making incompetent efforts to incorporate into his system whatever it would give the physicists the most pleasure to find there. The quaternion $(0 + \mathbf{a})\ (0 + \mathbf{b})$ can like any other quaternion be expressed as the sum of a scalar part and a vector part, and what we have to remember is that Hamilton did not define two distinct products of two vectors ; he defined one product only and interpreted its two parts. The descriptions ' scalar product ' and ' vector product ' are not to be found in the early expositions of quaternions, even as abbreviations.

To a great extent Hamilton's most fervent disciples were the most to blame. Longing to see quaternions used, they said in effect ' Never mind about following Hamilton through the logic of the two great works, of which practical men like you cannot be expected to see the beauty. We will tell you the rules, quite briefly. You call this function the scalar of **ab**, that function the vector of **ab**, you add them together, and there you have the product itself.' Naturally the practical men retorted by saying that if it was just a matter of calling names, they preferred to keep their names and their symbols for the functions they really wanted to talk about, and they saw no point in taking over a peculiarly tiresome negative. Moreover, what was meant by adding a scalar and a vector ? No operation was known to them which merged the two together, and if addition was only recording them side by side on the same line of the paper, with a plus sign that might as well be a comma, still less could there be any need for the scalar to be negative rather than positive. Looking back we see that this dispute about whether $rs\cos\varepsilon$ or $-rs\cos\varepsilon$ was right was meaningless. But it is not only in mathematics that titanic battles of the past are seen to have been unprofitable squabbles, and when we ourselves are near to losing our tempers, we do well to remember that very soon we in turn shall be judged in retrospect.

In contrast with the theory of quaternions, vector algebra has been from the first notationally a nightmare. In 1907 two Italian enthusiasts, Burali-Forti and Marcolongo, felt that the time was ripe to collate all the proposals that had been put forward, and to urge mathematicians to make a final choice. Their elaborate collection defeated its own purpose, for they

despaired of securing a working majority for any one of the candidates and made confusion worse confounded by putting forward a scheme of their own, which naturally has received no wider support than any of the others. In 1926 an unattractive notation of the author's design prevented Campbell's posthumous *Differential Geometry* from having the slightest chance of being judged on its merits as geometrical exposition. To-day we cannot say that any one system commands universal assent, but the number of serious competitors is not large, and we have acquired a sense of proportion or of humour. Graustein, for example, after describing the system in which his *Differential Geometry* is written, adds ' There are other notations which are entirely adequate. If the reader is already familiar with one of them, I can assure him from personal experience that he will have no difficulty in translating the notation of Study into his own.' The position is not wholly satisfactory, but it is not the user of vectors who is the worst sufferer, for as Graustein suggests one can soon get used to translating ' round ' and ' square ' into ' dot ' and ' cross ' or vice versa as one reads. The real trouble is that when we need a non-literal symbol in operational calculus or topology or logistics or any other part of mathematics, there is hardly a typographical sign in existence that we can propose without being told that it is somebody's favourite for the scalar-product stakes. Agreement on two or three symbols in vector algebra would liberate the whole keyboard.

With the ideography of geometry I am not concerned. We are familiar with the idea of a variable point, and whenever we write M_P for the projection of P on a fixed line we are using a functional notation, with P as independent variable and M as dependent variable. We are ready to express the dependence of the current point of a curve on a parameter t by denoting the point by O_t, but we are unaccountably shy of taking the next step and using the whole notation of the calculus with O as a functional symbol. The derivative dO/dt is the velocity of O, a tangential vector whose magnitude is the speed of O. This derivative has all the properties which the notation suggests. A function $O(u, v)$ of two variables is the current point of a surface. If u, v are made functions of one variable, the point describes a curve on the surface. The formula

$$\frac{dO}{dt} = \frac{\partial O}{\partial u}\frac{du}{dt} + \frac{\partial O}{\partial v}\frac{dv}{dt}$$

involves not only the existence of the tangent plane but also the classical formulae for the arc-element and for the angle between two curves. The formula

$$\frac{\partial^2 O}{\partial u\,\partial v} = \frac{\partial^2 O}{\partial v\,\partial u}$$

expresses through its normal component the symmetry of the bilinear curvature and through its tangential component the relation between geodesic curvature and the metric of the surface. It is true that the deductions are the same if we replace the symbol O by a symbol for the vector which locates O from some fixed origin, but what puzzles me is that this substitution should be thought necessary or even natural ; the use of O itself is a perfect example of notation at its best.

The most far-reaching innovation in formal symbolism in the present century was Einstein's summation convention. To replace $\Sigma a_r b_r$ by $a_r b_r$ at the cost of losing this notation for the simple product seems at first to be running a foolhardy risk for a trifling economy, but as we all know now the economy is tremendous and it is the risk which is negligible. The foundations of the tensor calculus had been well laid long before the convention was introduced, but the novelty of the calculus was in its ideas, not in its symbols ; there is no difference of principle between the use of a bracketful of numbers by Christoffel to identify a combination of derivatives and the use of a bracketful of numbers by Schläfli to identify a polytope. The summation convention reduced to manageable proportions the labour of applying the tensor calculus to particular problems ; Eddington has formulae with 24 dummy suffixes, and to handle such formulae with explicit summation signs would have taxed even his industry.

The summation convention has affected profoundly the presentation of results in coordinate geometry. A formula that is essentially explicit may consist of only a term or two and may be independent of the number of spatial dimensions

concerned. In consequence, the value of special notation such as that of Clebsch has been reduced almost to vanishing point; we hesitate to learn a language, however elegant, if there is nothing to be said in it that can not be said as briefly in terms universally understood. Even vector algebra tends to become absorbed, for there is no reason for inventing a special symbol for the scalar product of two vectors (a_1, a_2, a_3), (b^1, b^2, b^3) if the theorem that the value of this product is $a_r b^r$ provides us with a symbol almost as compact as any we could invent.

To exploit the summation convention to the full we make much use of such symbols as the Kronecker deltas, coefficients whose only possible values are 0 and 1 or in some cases 0, + 1, and − 1; these symbols take over the task of defining a pattern, leaving to other symbols the distinct function of specifying the elements of which the pattern is composed. A similar task is performed for functions of a real variable by the Heaviside unit operator $H(x)$, defined as a function whose value is 1 for positive values of x and 0 for negative values of x; instead of saying that y is $f(x)$ for $x > a$ and $g(x)$ for $x < a$, we can define y as $f(x)H(x-a)+g(x)H(a-x)$; it is open to us to define $H(0)$, but usually we have nothing to gain by doing so.

The summation convention operates within the field of the traditional notation of mathematics. The revolutionary change of the last half-century is the admission of logical symbols. These symbols compose a powerful shorthand, but it is not for this reason that they have forced an entry. Whatever rough notes may look like, shorthand has never been acceptable in print to the highest ranks of mathematical society; the $\because$, $\therefore$, $\angle$, $\|$ of the schoolbooks were anathema in journals and treatises. If the *Ausdehnungslehre* was slow to find advocates outside the author's family circle, that was because while it was easy to see that its geometrical multiplications and additions gave compact instructions for drawing figures, it was hard to see that the symbolism provided a calculus of any sort. For the same reason, a curious *New Analysis of Plane Geometry* by my contemporary A. W. H. Thompson, published in 1914, was the dampest of squibs in spite of the care given to its manufacture; the notation was adequate for descriptive purposes, but did not itself do any of the subsequent work.

The principle of a logical calculus goes back to Leibniz, but Leibniz did very little towards the actual formulation of his universal language. The deliberate and sustained effort to create this language was made by Peano, who with a band of enthusiastic helpers set to work around 1890 to express symbolically the fundamental ideas and theorems of all branches of mathematics. The driving force within Peano was antinationalism. He produced a quadrilingual mathematical dictionary, and he exerted himself to promote the use of uninflected Latin as an international language. For the inevitable reasons the language did not catch on; to a scholar whose native tongue was romance, or to one already accustomed to read French or Italian, the meaning of a sentence in Interlingua was no more and no less obvious than the meaning of the equivalent sentence in a living language; to the rest of the learned world, German or Russian, Indian or Japanese, nothing was being offered except a new foreign language, and a tacit but none the less irritating reminder either that they were newcomers to civilisation or that they were expected to merge their native culture into that of south-west Europe.

Peano's translation of mathematical enunciations into a universal symbolism has had far more lasting effects, less perhaps on account of his direct influence than because Russell and Whitehead found in the several editions of his *Formulaire de Mathématiques* the model that they needed for *Principia Mathematica*. Peano himself explains that while ideography, as he calls the combination of logical and mathematical notation, when called upon only to reduce chains of argument to manageable lengths, is properly described as useful, in the analysis of the fundamental principles of arithmetic and geometry it is indispensable. This is the unanswerable case for symbolism in logic. If an argument is conducted in language in which the words are everyday words, the argument tends to have the fringe of uncertainty which hangs round and adorns the words we use. The validity of my argument depends only on the consistency with

which I stick to the meanings of my words, but my conclusion may still convey a wrong impression to a reader. If I say 'The proposition p implies the proposition q,' I cannot rely on the listener to expand this mentally to the form 'His statements p and q are what he means by propositions, and the first of these implies the second, in the sense in which he talks of implication,' but this expanded assertion is the only one which can be open to logical criticism. If I express the whole matter in symbols whose significance I have explained, the reader may still say 'These are not what Descartes would have called propositions, and Poincaré would not have accepted this notion of implication,' but he is not for that reason in any doubt as to the meaning I am trying to convey.

So Peano's shorthand found its way into *Principia Mathematica*, and became known at second hand, with the more elementary of Whitehead's additions, to many mathematicians to whom the *Principia* itself was a closed book. It was clear that the lecturer on analysis was being offered systematic abbreviations for forms of words he was continually employing: 'for any ε, there is an n such that . . .', 'if $|z| < R$, then . . .'. What is much more important, the process of setting down the clauses in symbols is a clarifying process. For example, 'whatever the value of x, given ε there is an n such that . . .' is patently not the same as, 'given ε, there is a value of n such that, whatever the value of x, . . .'; if we have merely said 'because the sequence is always convergent . . .', we have not *forced* ourselves to ask which of the two assertions we are making, and in the course of an involved argument we may jump disastrously from one to the other, but we cannot express the condition symbolically at all unless we have recognised the distinction. The concept of uniformity, in all its many forms, does not require a Stokes to discover or an Osgood to expound. The logical symbols even introduce elements of a calculus. In a simple case it is childish to insist that the alternative to '$p(x)$ is always true' is not '$p(x)$ is never true' but 'sometimes $p(x)$ is false,' but if we have six or eight variables, each with its own possible range, it may be very hard to bear in mind the true contrary of a complicated existence theorem. 'It is a profoundly erroneous truism . . .' says Whitehead 'that we should cultivate the habit of thinking of what we are doing. The precise opposite is the case. Civilization advances by extending the number of important operations which we can perform without thinking about them.' Or to quote an author even better known to you: '"Oh, Mrs. Oliver, it must be wonderful to write." Mrs. Oliver rubbed her forehead with a carbonny finger and said, "Why?" "Oh," said Rhoda, a little taken aback. "Because it must. It must be wonderful just to sit down and write off a whole book." "It doesn't happen exactly like that," said Mrs. Oliver. "One actually has to *think*, you know."' We cannot but agree with Agatha Christie: 'Thinking is always a bore.' The best mathematicians are the laziest, who allow their notation to do all the drudgery, and logical symbols are the most efficient of drudges.

While logical symbols are slowly permeating the older branches of mathematics, newer branches are tending more and more to ideography. In topology, in group theory, and in algebra in the modern sense, theorems which would have been enunciated verbally half a century ago are now enunciated symbolically. For a few pages the effect is pretentious; the reader is likely to react with 'After all, you are only saying so-and-so.' But gradually the notation performs its work, directing the emphasis to the right place, reducing a long-winded corollary to a brief formula, rendering accessible regions which it would have been humanly impossible to explore with words alone. These developments belong, like the umbral notation of the algebra of invariants, to groups of specialists rather than to the whole community of mathematicians, but it is the freedom of the community from conventional restraints in the use of notation that has made it possible for the groups fully to satisfy their own needs.

There is no valid distinction between logical symbols and the symbols of mathematics, and a symbol thought of as logical may enter into a formula and provide a formal solution of a problem that is entirely mathematical. Among the symbols brought explicitly into formulation by the logicians is one for substitution. The idea

is not new to mathematics. On the contrary, if I say 'Apply Menelaus' theorem to the triangle *SKT* with the transversal *BQV*,' I am in effect substituting *SKTBQV* for *ABCLMN* in some earlier enunciation of the theorem. In using substitutions as examples in the theory of groups, we necessarily bring an appropriate symbol into our enunciations, but elsewhere most of the uses of the symbol are ideographic. Here is however an instructive example in which a symbol for substitution resolves an old difficulty. If a vector $\mathbf{r}$ has components x, y, z in a moving frame, we all know that the components of $d\mathbf{r}/dt$ are not $\dot{x}, \dot{y}, \dot{z}$; the vector $d\mathbf{r}/dt$ is obtained by adding to the vector whose components are $\dot{x}, \dot{y}, \dot{z}$ a certain vector product $\boldsymbol{\Omega} \times \mathbf{r}$. The formula

$$d\mathbf{r}/dt = (\dot{x}, \dot{y}, \dot{z}) + \boldsymbol{\Omega} \times \mathbf{r}$$

is offensive, since the second term involves reference to a particular frame and the other terms do not, but how otherwise is the vector $(\dot{x}, \dot{y}, \dot{z})$ to be described? The answer is provided by a substitution symbol. If $\mathcal{A}$, $\mathcal{B}$ are two congruent rigid bodies, and $\mathbf{r}$ is any vector, there is a vector $\mathbf{s}$ whose specification in any frame that is attached to $\mathcal{B}$ is the same as the specification of $\mathbf{r}$ in the congruent frame attached to $\mathcal{A}$; let $\mathbf{s}$ be denoted by $(\mathcal{B}/\mathcal{A})\mathbf{r}$. In the case of the moving frame, this is a frame $\mathcal{F}_t$ associated with the variable t. To say that the frame is rigid means that there is a frame $\mathcal{F}$ independent of t with which $\mathcal{F}_t$ is always congruent. The vector whose components in $\mathcal{F}$ are (x, y, z) is the vector $(\mathcal{F}/\mathcal{F}_t)\mathbf{r}$, and because $\mathcal{F}$ is fixed, the vector whose components in $\mathcal{F}$ are $(\dot{x}, \dot{y}, \dot{z})$ is the rate of change of $(\mathcal{F}/\mathcal{F}_t)\mathbf{r}$, and we return to the vector whose components in $\mathcal{F}_t$ are $(\dot{x}, \dot{y}, \dot{z})$ by the operation $\mathcal{F}_t/\mathcal{F}$. Thus the intrinsic formula for the rate of change of a vector given in relation to a moving rigid body $\mathcal{F}_t$ is quite simply

$$\frac{d\mathbf{r}}{dt} = \frac{\mathcal{F}_t}{\mathcal{F}}\left\{\frac{d}{dt}\left(\frac{\mathcal{F}}{\mathcal{F}_t}\mathbf{r}\right)\right\} + \boldsymbol{\Omega} \times \mathbf{r}$$

With this example I conclude. I should have liked to give you a picturesque narrative, illustrated by strange pages from Byrne and Dodgson and Frege, and full of high principles, deep philosophy, and sage counsel, but that was beyond my power. I have no peroration, and if I am expected to adorn my tale I have only the depressing moral to draw that if the evil that men do lives after them, the timelessness of mathematics makes our mistakes immortal. The iniquities of Pope John XXIII survive only in the delight with which each new reader tastes the relish of Gibbon's matchless comment on them, and no-one would be so bold as to say that the world to-day would be happier to live in if King George III had been less obstinate, but to the remotest future civilised mankind will be suffering because of the deference paid by a few great men to a suggestion thrown out by the well-meaning and otherwise little-to-be-remembered Mr. William Jones.

MODERN DEVELOPMENTS IN CARBOHYDRATE CHEMISTRY

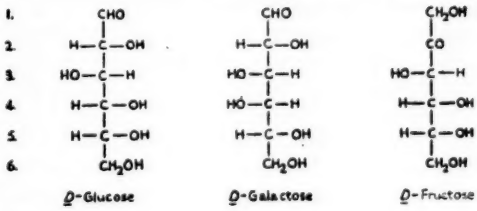
Address by

PROF. E. L. HIRST, F.R.S.

PRESIDENT OF SECTION B

THE period between the meetings of the British Association in Birmingham in 1913 and in 1950 has been one of great activity in the investigation of the chemistry of natural products. Astonishing progress has been made and many new chapters have been added to the text-books of organic chemistry. This is particularly the case with the sugars and it is the object of our opening session to review briefly the main lines of advance by which the present position in structural carbohydrate chemistry has been reached and to indicate some of the problems which still await solution. Such a review seems particularly appropriate at this meeting since so much of modern carbohydrate chemistry has emanated from the laboratories of the University of Birmingham from the time when the school of carbohydrate research was established there in 1925. It had been hoped that the founder of this school, Sir Norman Haworth, F.R.S, who was a distinguished past President of this Section, would take part in our proceedings to-day and we regret poignantly that his recent death has deprived us of the presence of one who was himself responsible for so many of the important advances made during the past thirty-five years.

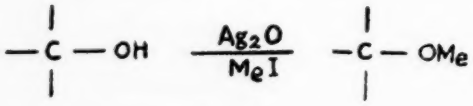
It is of interest at the outset to recall briefly the problems which faced those who were active in the investigation of sugar chemistry forty years ago. By that time the researches of Emil Fischer and his school had established, by work unexcelled for brilliancy of imagination and experimental skill, the stereochemical relationships of the simple sugars and it had become clear that in their normal derivatives these sugars possessed ring structures and did not exist in the open chain form as aldehydes or ketones. Much information had been collected concerning the behaviour and properties of the sugars and their derivatives, but no proofs of ring structures were available and little was



known with certainty of the way in which the sugar residues were disposed in the di- and tri-saccharides. Ignorance of the molecular structure of the complex polysaccharides such as the celluloses, starches, gums and mucilages was much more profound. Problems of the most formidable nature, for the solution of which no known methods were available, faced alike the organic chemist interested in structures, the biochemist concerned amongst other major issues with nutrition, muscular contraction, fermentation and chemotherapy, and the industrialist who required fundamental information for developments in textiles, paper-making, brewing, explosives and the many other manufacturing processes based on the chemistry of the carbohydrates.

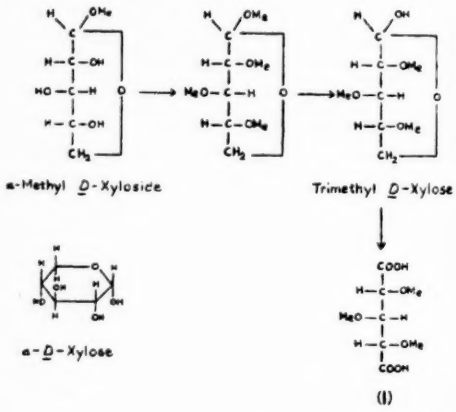
One of the main reasons for the comparatively slow progress in sugar chemistry in the early years was the lack of an accurate knowledge of ring structure. It had been assumed, largely by analogy with the stable γ-lactones of hydroxy acids, that a similar five-membered ring occurred in the normal sugars, but as knowledge of

the properties of sugar derivatives increased many anomalies appeared and there followed a period of confusion and uncertainty. Nevertheless, as so often happens, the need was soon met and at the time when our review commences, a most powerful weapon for the attack on structural problems was being forged in the laboratories of the University of St. Andrews where the work of Purdie and Irvine led the way in the development of modern carbohydrate chemistry. The



origin of this lay in Purdie's observation that by the action of silver oxide and methyl iodide on lactic acid both the carboxyl and the hydroxyl groups underwent methylation. The reaction with alcoholic groups was found to be a general one and in this way it became possible for the first time to label with easily recognisable groups the free hydroxyl groupings present in carbohydrates. Since these methyl ethers resisted chemical action during subsequent operations, a method was provided whereby the points of junction of the substituents in a sugar molecule could be detected. In order to make use of this procedure it was necessary to have ready as standard reference substances a series of partially methylated derivatives, for example, 2 : 3 : 6-trimethyl glucose, in which the position of the methyl groups was known with certainty, and much pioneering work in this field was carried out in the St. Andrews laboratories.

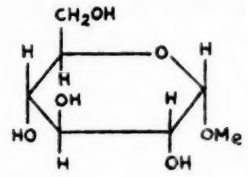
A further stage in the progress of the structural investigations lay in the application of the methylation method to the determination of the nature of the rings present in the sugar molecules. It was used (Hirst), to ascertain the ring structure of the stable α- and β-forms of methylxyloside, which are obtained from xylose by the action of methyl alcohol in the presence of hydrogen chloride. On methylation by Purdie's reagents, or by the process subsequently developed by Haworth, using dimethyl sulphate and sodium hydroxide, the fully substituted methylxylosides could be prepared without risk of any change in the nature of the ring system and trimethyl xylose was readily obtainable from these by acid hydrolysis. This trimethyl xylose on oxidation with nitric acid gave the internally compensated optically inactive trimethoxy glutaric acid (I) and it followed that the ring system present in normal methylxyloside must be a six-membered one containing an oxygen bridge between the first and the fifth carbon atoms. For this type of ring Haworth subsequently proposed the designation pyranose. An elaboration of the method fixed the ring structure in α-methylglucoside, since on methylation this gave rise to a tetramethyl glucose, which on oxidation with nitric acid yielded the same inactive trimethoxyglutaric acid (I). It therefore became evident that the normal derivatives of glucose contain a six-membered pyranose ring instead of the five-membered ring which had previously been assumed. The way was then clear for a general attack on the problem of



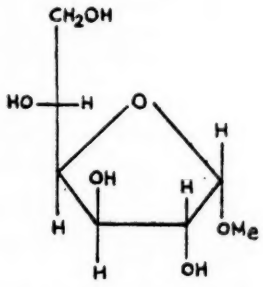
ring structures in simple and compound sugars.

From 1925 onwards the Birmingham school under Sir Norman Haworth's leadership took a prominent part in this work which led to broad generalisations and provided a great simplification in sugar chemistry. The ordinary stable varieties of the sugars and their methylglycosides were found to possess the pyranose structure whilst the unstable, readily hydrolysable methylglycosides, of which Fischer's 'γ'-methylglucoside is a typical example, possessed the five-membered furanose ring.

The ring structures of the monosaccharides had now been settled and as the years went by confirmatory methods of proof became available in many instances.

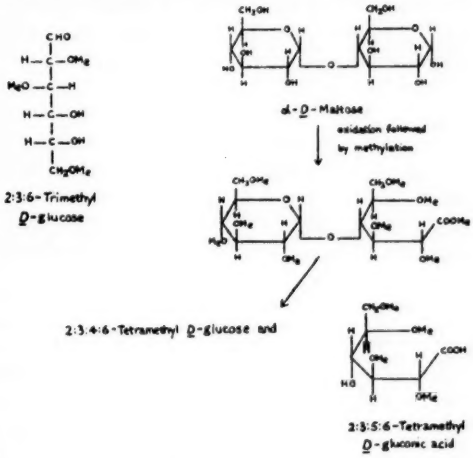


α-methyl D-glucoside



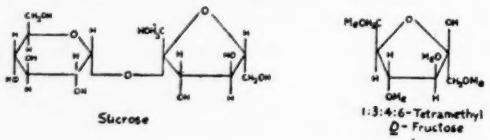
α-methyl D-glucofuranoside

Simultaneously efforts were made to determine the ring structures present in the disaccharides, but the problems here are more complicated since it is necessary to determine (a) the nature of the ring present in each of the two residues, (b) the point of junction between the residues, (c) the nature of the glycosidic group involved. Now in the case of the disaccharides methylation followed by hydrolysis frequently gives only a partial answer to the problem of structure. For instance the fully methylated derivative of maltose yields on hydrolysis an equimolecular mixture of 2 : 3 : 4 : 6-tetramethyl glucopyranose and 2 : 3 : 6-trimethyl glucose. Inspection of the formula of the latter substance shows that free hydroxyls on both C_4 and C_5 are available for the engagement of the ring and it follows that the isolation of this particular trimethyl sugar leaves open two possibilities for the ring form of the corresponding glucose residue as it occurs in the disaccharide. Further evidence is required before a choice can be made between the two alternative structures for the disaccharide which might be either 5-glucopyranosido-glucofuranose or 4-pyranosido-glucopyranose. The workers in the Birmingham school developed an ingenious method for overcoming this difficulty. If instead of the free sugar the open chain derivative maltobionic acid is subjected to methylation, every position in the residue which originally carried the reducing group is now available for substitution, except that carbon atom which is involved in the junction of the two glucose residues. Hydrolysis of the fully methylated maltobionic acid will therefore yield in addition to tetramethylglucopyranose a tetramethyl gluconic acid which possesses one free hydroxyl group. The position of this hydroxyl group fixes the point of junction of the two residues and hence, by taking into account also the isolation of 2 : 3 : 6-trimethyl glucose in the direct methylation experiments, the nature of the ring is fixed also. In this way maltose was proved to contain two pyranose residues linked through positions 1 and 4, and other evidence indicated that an α-glucosidic link was involved. By suitable variations of the procedure the structures of lactose (4-β-galactopyranosido-glucopyranose), cellobiose (4-β-glucopyranosido-glucopyranose), gentiobiose (6-β-glucopyranosido-glucopyranose), and of other disaccharides



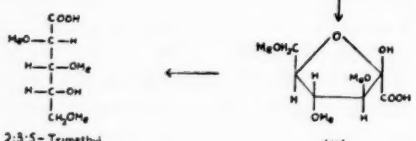
and trisaccharides were established. Sucrose (α-glucopyranosido-β-fructofuranoside), one of the most important of the disaccharides, presented special difficulties and some time elapsed before the structure of the fructose portion was established. The clearest proof that the ring structure is of the furanose type was

provided by a study of the action of permanganic acid on the substance (II)



Sucrose

1:3:4:6-Tetramethyl *D*-Fructose



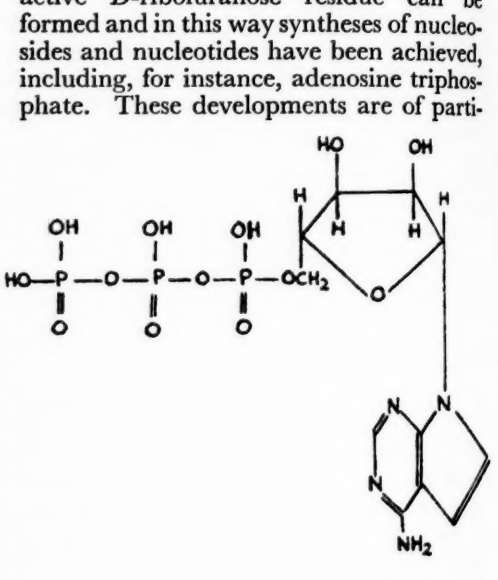
2:3:5-Trimethyl *D*-arabonic acid

(II)

obtained by the mild oxidation of tetramethyl fructose. Elimination of carbon dioxide took place and 2 : 3 : 5-trimethyl-*D*-arabonolactone was formed. Sucrose has, therefore, the structure shown above, rotational evidence obtained during hydrolysis experiments giving strong indications that the molecule contains an α-glucosidic and a β-fructosidic group.

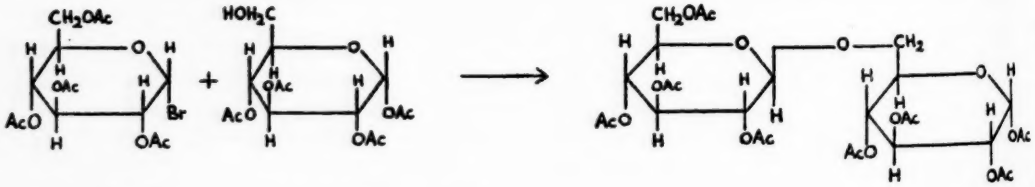
In many cases these proofs of structure have been amplified by syntheses and the formulæ given below outline the steps in a typical case, namely the synthesis of gentiobiose (Helferich). It is curious, however, that sucrose has resisted all attempts to synthesise it by purely chemical methods. Despite the most intensive work no trace has yet been found of natural sucrose when the combination of glucose and fructose possessing the correct ring structures has been effected, although an iso-sucrose, differing from the natural product only in the nature of the glycosidic links, can be obtained in this way. Recently it has been found by Hassid and Doudoroff that α-*D*-glucose 1-phosphate and *D*-fructose give natural sucrose in good yield under the influence of an enzyme present in the bacterium *Pseudomonas saccharophila*, an enzymatic synthesis of cane sugar being thus achieved.

The substances we have been considering are O-glycosides. Nitrogen analogues exist also, formed by the union of a sugar with an appropriate amine, and in recent years synthetic work of outstanding importance has been accomplished in this field by the Cambridge School under the leadership of Prof. A. R. Todd, F.R.S. Methods have been elaborated whereby N-glycosides containing the highly reactive *D*-ribofuranose residue can be formed and in this way syntheses of nucleosides and nucleotides have been achieved, including, for instance, adenosine triphosphate. These developments are of particular importance in connection with the chemistry of the biologically significant nucleic acids and co-enzymes.



Adenosine Triphosphate

Many of the examples we have considered up to the present have served to illustrate the power of the methylation method, which still remains one of the most valuable of all those available to the



Aceto bromo-*D*-glucose

1:2:3:4-Tetra-acetyl *D*-glucose

Octa-acetyl gentiobiose

sugar chemist, but the carbohydrate molecule is so labile and so versatile in its behaviour and the problems it poses are so complex that the widest possible variety of physical and chemical methods are now required for the determination of structure. It will be recalled, for instance, that evidence for the relative dispositions of the α- and β-groups in glucose has been sought by methods so widely divergent as conductivity measurements in borax solution and X-ray crystallographic analysis. The comparative rapidity with which the chemistry of ascorbic acid and its derivatives was worked out depended upon X-ray and ordinary crystallographic measurements and on the study of optical rotatory dispersion in the ultra-violet region, in addition to the classical methods of organic chemistry and novel applications of the methylation process.

(i) $R{\cdot}CHOH{\cdot}CHOH{\cdot}R \xrightarrow{HIO_4} R{\cdot}CHO + OHC{\cdot}R$

(ii) $R{\cdot}CHOH{\cdot}CHOH{\cdot}CHOH{\cdot}R^1 \xrightarrow{HIO_4} R{\cdot}CHO + OHC{\cdot}R^1 + HCOOH$

(iii) $CH_2OH{\cdot}CHOH{\cdot}R \xrightarrow{HIO_4} H{\cdot}CHO + OHC{\cdot}R$

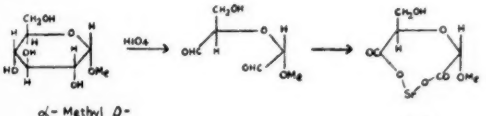
(iv) $CH_3{\cdot}CHOH{\cdot}R \xrightarrow{HIO_4} CH_3{\cdot}CHO + OHC{\cdot}R$

Of other reagents which have found important uses in carbohydrate chemistry, periodic acid is of special significance and the limits of its usefulness have by no means been reached. Like lead tetra-acetate, this substance has the property of attacking glycols which contain hydroxyl groups on contiguous carbon atoms, the carbon-carbon bond being broken with the formation of two aldehyde groups. The use of periodic acid was first introduced by Malaprade, to whom sugar chemists in particular owe a special debt of gratitude, and its application to carbohydrate problems was developed extensively by C. S. Hudson and his colleagues in the U.S.A. and by numerous workers in this country. One or two examples chosen from widely differing problems may be given to illustrate the versatility of the reagent.

(a) Sodium periodate attacks the free sugars, in many instances quantitatively, liberating formic acid, and so providing a convenient and accurate method for the micro-analytical determination of the sugars. Under appropriate conditions terminal $—CH_2OH$ groups yield formaldehyde and $CH_3{\cdot}CHOH—$ groups acetaldehyde, both reactions finding important analytical uses.

(b) It may often be employed to establish the position of the methyl groups in partially methylated sugars. For instance, an unknown dimethyl mannuronic acid, which was encountered in the course of work on alginic acid, reacted with periodic acid yielding a methylated aldehyde, and the latter on further oxidation gave *i*-dimethoxy succinic acid. The original substance was, therefore, the 2 : 3-dimethyl derivative of mannuronic acid.

(c) The problem of the disposition of the methoxyl groups round the first carbon atom of the α- and β-forms of the methylglycosides has been studied by the use of periodic acid. In the case of α-methyl-*D*-glucoside many independent lines of evidence point to the arrangement depicted below in which the —OMe group is situated beneath the plane of the ring. By means of an ingenious application of the use of periodic acid it has been confirmed that this spatial arrangement holds generally for all α-methyl-*D*-hexopyranosides and hence also for the α-forms of the sugars. During the oxidation of the methylglycosides a dialdehyde is formed and on further oxidation with bromine water this readily gives the corresponding acid which can be isolated as its strontium salt (III). In this the original oxygen bridge is still intact, but the optically active centres at C_2, C_3 and C_4 have disappeared. All α-methyl-*D*-hexopyranosides should, therefore, give the same product and this is found to be the case (Hudson and Jackson).

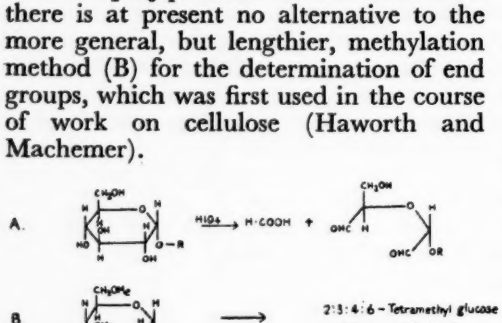


α- Methyl *D*-glucopyranoside (III)

In a similar way β-methylglycopyranosides of the *D*-series, for which the arrangement of the groups round C_1 is enantiomorphic to that present in the α-derivatives, give

rise to a product which differs from (III) only in respect of the spatial arrangement at C_1.

(d) One of the most valuable applications of periodic acid is for the detection and estimation of terminal sugar residues which are present in complex carbohydrates. As a simple example let us consider a polysaccharide of the structure shown in (A) in which there is a terminal glucose residue, attached to the remainder of the molecular structure R, which contains no other terminal groups, reducing or non-reducing, and no residues linked exclusively through their C_1 and C_6 positions. When such a molecule is oxidised by periodate it is clear that the terminal residue will give rise to formic acid, the amount of which enables a direct calculation to be made of the proportion of the end group in the molecule. Wherever conditions are suitable, this method is particularly convenient since it can be carried out on the micro-scale, permitting end-group determinations to be made in a day or two with only milligrams of material. It has been used extensively to supplement the methylation method for the assay of end groups in amylopectins and glycogens, and other polysaccharides (Halsall, Hirst and Jones ; Hassid ; Meyer ; Bell). Complications arise when the molecule contains free reducing groups or chains of hexose residues linked solely in the C_1-C_6 positions and in such cases there is at present no alternative to the more general, but lengthier, methylation method (B) for the determination of end groups, which was first used in the course of work on cellulose (Haworth and Machemer).



(e) Another promising development of the periodate method is its use in the study of complex polysaccharides which contain many branched chains, such as are found in amylopectin, glycogen and in plant gums and mucilages (Hirst, Jones). We have seen that a terminal hexopyranose residue is attacked with liberation of formic acid. On the other hand a hexopyranose residue which is linked to others solely through its C_1 and C_4 positions will be oxidised to a dialdehyde by periodate, whereas if the links are through C_1 and C_3, the residue will remain unattacked. Again, if a branch chain is attached at either of the positions C_2 or C_3 in one of the residues of a long chain of C_1-C_4 linked residues, treatment of the substance with periodate will result in the oxidation of all the sugar residues except that one in which the point of branching occurs. If the oxidised polysaccharide is then hydrolysed, the nature and the amount of the unattacked sugar residues may be determined. Such methods have already proved their utility in structural work on the starches and gums and it may be expected confidently that their further elaboration will permit an insight to be gained into structural features which are difficult if not impossible to deal with by any of the other methods at present available.

Of the more recent developments, the application of the methods of chromatography to carbohydrate chemistry is of outstanding importance. Throughout the history of sugar chemistry one of the major difficulties has always been that of the qualitative and quantitative separation of mixtures of sugars. Generally speaking these substances resemble one another so closely in physical and chemical properties that separation is always difficult and frequently has to depend on some specific reaction which is, unfortunately, rarely quantitative. Nevertheless, a great deal has been accomplished on these lines, and if sufficient material is available it is usually possible to identify the components of a mixture. These methods however demand large quantities and progress in important investigations has been retarded by the difficulty of securing materials on an adequate scale. Similar problems are encountered in the study of polysaccharides by the methylation method when the hydrolysis of the fully methylated derivative may give rise to a complex mixture of monosaccharides of different kinds and of various degrees of methylation. Much can be done by preferential extraction from solution by immiscible solvents

and by fractional distillation under diminished pressure, but it became clear some years ago that the limits of usefulness of these methods were being reached and there was urgent need for a new approach. This has been provided by partition chromatography (Martin and Synge), which has developed with amazing rapidity in the sugar field since Partridge first applied the filter-paper-strip technique to the separation of simple sugars, using solvents such as butanol-water mixtures for the development of the chromatograms. It would be inappropriate to discuss the methods in detail at this discussion, since another of the sessions of Section B is to be devoted entirely to chromatography, but it is important to emphasise the value of this new tool in carbohydrate research. Many workers have contributed to this development and particular reference may be made to the advances made in this country by J. K. N. Jones and his collaborators who have shown that complex mixtures of sugars, including their methylated derivatives, can be separated and determined quantitatively on a scale of milligrams or, in favourable cases, of micrograms. Furthermore, by the use of columns of powdered cellulose and an automatic device for collecting fractions of the eluate, these workers have succeeded in carrying out quantitatively the separation of mixtures on a larger scale, obtaining a sharpness of separation which had hitherto been quite impossible to achieve. Both in sensitivity and in range the chromatographic methods far exceed the earlier procedures and for the time being the limiting factor in the structural study of the complex carbohydrates resides not so much in the separation and identification of the various sugar residues as in the difficulty encountered in hydrolysing the polysaccharides without damage to the component sugars.

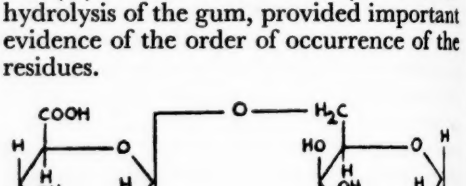
The methods described in the preceding sections enable the chemist to obtain much information concerning the nature of the sugar residues in a polysaccharide and the precise mode of linkage of these units, but only in the simplest cases do they permit the assignment of a unique molecular structure. In many polysaccharides several different sugars are found and when it is recalled that each of these may exist in more than one ring form and can be united to other residues at one or more of several positions and by either α- or β-glycosidic links, it will be realised that the problems involved are far from simple. Indeed they approach in complexity those encountered in protein chemistry which also has to deal with the order in which the numerous residues are bound together in the repeating unit of a high polymer. And even when the detailed structure of the repeating unit is known there remains the difficulty of determining the size and shape of the large molecule, a difficulty which is increased by the tendency of many of these substances to exist as mixtures of polymers containing molecules ranging widely in size. Much use has been made of osmotic pressure measurements for the determination of molecular weights and valuable information has been gained thereby, but the possibility of association in solution introduces uncertainty and the application of the method to polysaccharides is not easy. In simple cases chemical methods such as end group determination may be sufficient and of these we may cite both the methylation and the periodate oxidation methods for non-reducing terminal groups, and those which depend on the oxidation of a reducing terminal group. When the molecule is unbranched and takes the form of a long straight chain, the remarkable relationship between molecular size and viscosity discovered by Staudinger ($\eta_{sp}=K_mMc$, where η_{sp} is the specific viscosity of a solution of concentration c, M is the molecular weight and K_m is a constant) is of great value once the appropriate constant has been evaluated by osmotic pressure or other measurements. This method is so simple to apply that there is a tendency to make use of it in circumstances where it is inapplicable. Unfortunately its range is limited and it is of little help in the case of the interesting and important polysaccharides which contain branched chains. Other physical methods which afford evidence of the shape and the size of a molecule include X-ray analysis, electron microscopy, sedimentation in the ultracentrifuge and the recently developed method based on light scattering in solution. For technical reasons, however, these methods, with the exception of viscosity measurements, are at present difficult

to apply to polysaccharides and their derivatives, and there are still many gaps in our knowledge.

We may now consider briefly how these various methods are applied to the study of a typical polysaccharide. In the first place it is necessary to obtain the material in a pure state and we are at once confronted with a major difficulty. Only in very rare instances, such as cotton cellulose and some of the waxy varieties of starch, does nature provide an easily obtainable single polysaccharide in a state approaching chemical purity. Much more often do we encounter mixtures of several polysaccharides of closely related structure, as in the hemi-cellulose group, the pectins, mucilages and seaweed carbohydrates. In such circumstances it is essential to conduct the isolation and purification with the utmost caution and by methods which will preserve both the structure and the molecular size of the material which is to be examined. Once this has been achieved, analytical procedures are available by which the proportions of uronic acid, pentose and methyl pentose residues may be determined. After hydrolysis of the polysaccharide the liberated sugars can be identified and the proportions determined by chromatographic or other methods. Attempts to ascertain the shape and size of the molecule by physical methods may then be made. It is useful also to carry out oxidations with sodium or potassium periodate in order to ascertain whether formic acid, formaldehyde or acetaldehyde are liberated, and whether any of the sugar residues of the polysaccharide remain unattacked by the periodate. In this way much information concerning the mode of linkage of the sugar residues may be derived.

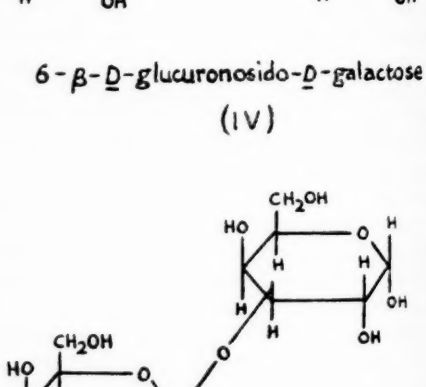
An effort to simplify the problem can then be made by taking advantage of the different rates of hydrolysis of the various sugar residues present in the molecule. In this way the more labile residues may be removed, leaving a resistant core which has a much simpler structure. Good examples of this are found amongst the gums and mucilages which often contain pentose residues which are readily removable on hydrolysis by dilute acid, leaving a polysaccharide consisting essentially of hexose and uronic acid residues. On further hydrolysis it often happens that a resistant acidic disaccharide unit is isolated, such as 6-β-*D*-glucuronosido-*D*-galactose (IV) from gum arabic and 2-β-*D*-glucuronosido-*D*-mannose from cherry gum (J. K. N. Jones). The structure of these aldobiuronic acids can be ascertained by the standard methods, and important evidence concerning the order and mode of junction of two of the residues thus becomes available. Similarly, by careful control of the conditions of hydrolysis it is sometimes possible to isolate a resistant disaccharide or a trisaccharide.

Recent examples are to be found in F. Smith's investigations on Gum Arabic when the disaccharides 3-β-*D*-galactosido-*L*-arabinose and 3-*D*-galactosido-*D*-galactose (V) which were isolated by controlled hydrolysis of the gum, provided important evidence of the order of occurrence of the residues.



6-β-D-glucuronosido-D-galactose

(IV)



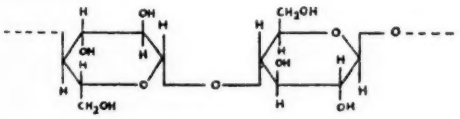
3-D-galactosido-D-galactose

(V)

Further information is sought by applying the methylation method both to the original polysaccharide and to the more

resistant portion which remains after partial hydrolysis. By hydrolysis of the fully methylated derivatives two sets of partially methylated monosaccharides are obtained, the free hydroxyl groups in which denote the points of linkage in the polysaccharide. A comparison of the positions of the free hydroxyl groups in the hydrolysis products from the degraded polysaccharide with those in corresponding residues derived from the original polysaccharide then provides further evidence of the mode of linkage of the residues. By piecing together all these items of information the general structure of the repeating unit of the polymer becomes evident even if, as is usually the case, a unique structure cannot yet be assigned.

Such then are the methods available and it is proposed in conclusion to summarise briefly the position now reached in assigning structures in the main groups of polysaccharides. It is in this field that we encounter the greatest complexity and unfortunately the trend of recent results permits of few generalisations which would enable a comprehensive picture to be drawn. The widest diversity is found amongst polysaccharides of biological importance in regard to the number and nature of the sugar residues, their mode of linkage and the types of branched chain structure present in the molecule. To represent diagrammatically the complex structures it will be convenient to use at times a shorthand notation by which, for example, $_4G_1$ stands for an α-*D*-glucopyranose residue united to other residues both by its glucosidic link and through C_4, and $_5^3A_1$ represents an arabofuranose residue linked through C_1, C_3 and C_5. Using this notation, cellulose, one of the principal components of the membrane which bounds the individual cells of both land and sea plants, may be written as $(G_1, (_4G_1)_n, _4G)$. The molecule consists of a long straight chain of β-*D*-glucose residues linked together through positions C_1 and C_4, the fuller representation of which is shown below. It is generally accepted that this depicts correctly the main features of the molecular structure and many of the properties of cellulose find a ready interpretation on this basis. Yet it must be admitted that it fails to satisfy fully either the academic or the applied scientist and the reasons for this are not far to seek. In cellulose we have to deal with a macromolecule of colloidal dimensions containing some 3,000 or more



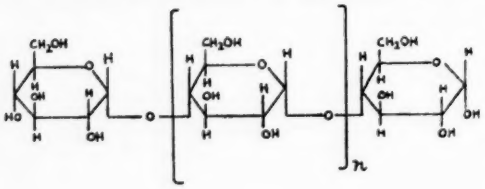
Cellulose

individual sugar residues. Many of the properties which are of the greatest importance in industry, such as the sensitivity to hydrolysis and to oxidation, appear to depend on a minute fraction of the total number of residues and for the detection of these centres it would be necessary for the chemist to deal with yields of a fraction of 1 per cent. It is indeed in the nature of a triumph for the organic chemist that he can already claim a knowledge of some 99·5 per cent. of the molecular structure, but the small remainder is of special importance in the behaviour of a macromolecule and the search continues, and it is not yet established whether the source of the instability is an inherent property of molecules above some limiting size or whether linkages of another type recurring at long distances apart in the chain of glucose residues are responsible.

A similar and as yet unexplained instability towards hydrolysis is encountered also in another of the glucose polysaccharides which have been intensively studied. This is the amylopectin component of the starch granule. Until a few years ago it was uncertain whether the carbohydrate portion of the granule was a single polysaccharide or a mixture but since 1940 it has become clear (Meyer ; Schoch ; Kerr ; Peat) that in general two components are present in intimate mixture. One of these, designated amylose, is structurally very like cellulose in being composed of unbranched chains of 1 : 4-linked glucopyranose residues, with an α-glucosidic link in place of the β-junction found in cellulose.

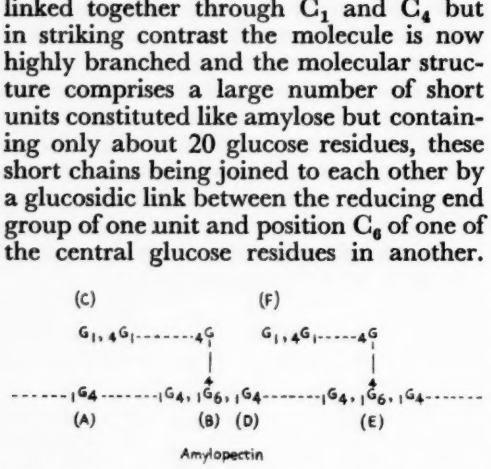
The length of the chain is from 100 to 500 glucose residues depending on the variety of starch. The amylopectin component is usually present in greater proportion in the starch granule and in the so-called

waxy varieties of starch the amylose content may be less than 1 per cent. The molecule of amylopectin is unusually large



Amylose

and may contain several thousand α-glucose residues. As in amylose these are linked together through C_1 and C_4 but in striking contrast the molecule is now highly branched and the molecular structure comprises a large number of short units constituted like amylose but containing only about 20 glucose residues, these short chains being joined to each other by a glucosidic link between the reducing end group of one unit and position C_6 of one of the central glucose residues in another.



Amylopectin

ABC, DEF are chains containing 20 1: 4-linked glucose residues. These units are joined together at B, E, etc., by 6 : 1 links. At C and F there are non-reducing terminal glucose residues.

Much of the detail of this picture remains to be filled in and it is possible that about 1 in 100 of the glucosidic links are of a different chemical type.

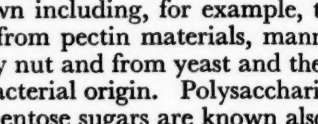
A purely diagrammatic representation of the principal features of the molecular architecture is shown above, which illustrates how amylopectin can act as a very long chain molecule possessing numerous short side chains.

In its main features glycogen is similarly constituted but here the repeating units are shorter and normally contain only 12 glucose residues. Much has been learned about starch and glycogen by enzyme studies and the synthesis of both amylose and amylopectin from glucose 1-phosphate has been achieved by means of enzyme systems present in potato juice. A further account of these remarkable developments will be given by Prof. S. Peat, F.R.S., in one of the subsequent papers of this discussion.

Polysaccharides containing fructose residues are of common occurrence, inulin from artichokes and dahlia tubers and levans from meadow grass and barley leaves being important examples. They are of comparatively low molecular weight and are built up essentially of chains of fructopyranose units, the linkage being C_1C_2 in inulin and C_2C_6 in the levan from meadow grass. Their structure is not yet fully understood however since it has recently been confirmed (Percival) that the inulin molecule contains in addition to fructose some 6 per cent. of glucose residues.

Many other hexose polysaccharides are known including, for example, the galactan from pectin materials, mannans from ivory nut and from yeast and the dextrans of bacterial origin. Polysaccharides based on pentose sugars are known also and one of the most important of these is the xylan which forms so large a proportion of the woody tissues of plants. It contains chains of β-xylopyranose units linked through C_1 and C_4, giving a structure reminiscent of that of cellulose.

Its molecular weight however is low, probably about 80 residues, and the present indications are that the molecule contains one point of branching, and possesses a structure of the type shown in the diagram (Chanda, Jones, Percival).



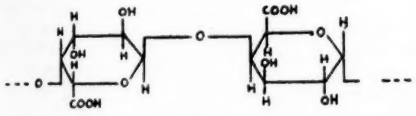
Probable structure of Esparto Xylan (X = *D*-xylopyranose residue).

In marked contrast we have the arabinose polysaccharide which accompanies galactan and pectin acid in the pectic materials of plants. This araban contains one arabofuranose end group for every three arabinose residues in the molecule. It is built up of equal numbers of the residues $A_1\ldots$, $\ldots{}_5A_1\ldots$ and $\ldots{}_5A_1\ldots$ and must, therefore, contain a many-

branched chain, one of the possible arrangements of which is shown below.

Araban (A = *L*-arabofuranose residue).

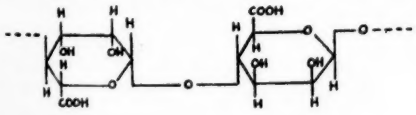
Many of the polysaccharides of biological importance contain oxidised hexose residues, notable glucuronic acid, found in bacterial polysaccharides, plant gums, etc., galacturonic acid present in pectins and plant mucilages, and mannuronic acid which is frequently found in the carbohydrate portions of seaweeds. The presence of these unstable residues introduces serious experimental difficulties which have not yet been fully overcome. Work has proceeded sufficiently far however to give us a general picture of the molecular architectures of pectic acid and alginic acid which may be cited as typical examples. Pectic acid which is the main constituent of pectins contains a chain of galacturonic acid residues linked in the 1 : 4 positions by α-glycosidic links (Jones ; Smith).



Pectic Acid

In the plant a proportion, which may be as high as 80 per cent. in some cases, of the acidic groups occur as the methyl ester, with profound effect on the properties which are of industrial importance.

Alginic acid also has great and increasing applications in industry. In some seaweeds, notably in *Laminaria sp.*, at certain times of the year it forms an astonishingly high proportion of the solid

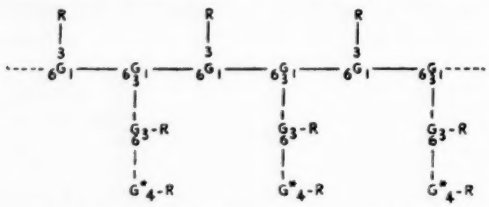


Alginic Acid

matter of the plant. Its structure resembles those of pectic acid and cellulose in consisting of long chains of 1 : 4 linked residues, the repeating unit in this case being a β-mannuronic acid residue. In the living plant its formation and disappearance are linked with transformations of the alcohol mannitol, and, as we shall hear in further detail from Dr. E. G. V. Percival, very complicated structural relationships are encountered amongst the seaweed carbohydrates.

The polysaccharides we have considered so far provide problems of quite sufficient difficulty, but they are to be regarded as simple in that only one type of sugar residue had to be dealt with. Much greater complexity is encountered when the molecule contains two or more kinds of residue. A great many polysaccharides of biological and industrial importance come within this category and the already formidable list is growing as investigations proceed into the nature of bacterial polysaccharides, hemicelluloses, plant gums and mucilages. Knowledge of the structure of substances such as the pneumococcal polysaccharides and hyaluronic acid is obviously important in medicine, and later on in this discussion Prof. M. Stacey, F.R.S., will deal with the chemistry of the bacterial polysaccharides and the fascinating biochemical problems involved in this field. In industry, only empirical data are available for controlling the use of such materials as gum arabic, gum tragacanth and gum guar, which are used on a large scale in textile finishing, paper making and other operations. In this field, however, structural work is only in the initial stages and it seems likely that for the full solution of the problems it will be necessary to develop new methods. Nevertheless, as the result of work carried out during the past dozen years, preliminary knowledge of the general structure of several gums and mucilages has been gained. These include gum arabic, gum tragacanth, gums from the damson and cherry trees, grapefruit and lemon gums and mucilages from numerous plant seeds and seaweeds and from the bark of the tree *Ulmus fulva*. They contain a bewildering variety of sugar residues united in almost all the available positions. In so far as any generalisation is yet possible it may be said that there is a tendency for the gums to contain a stable backbone of firmly linked galactose residues to which are attached various side chains containing uronic acid and pentose residues. Gum

arabic, the investigation of which has been carried further than is the case for most of the gums, may be cited in conclusion as an example of this. Some years ago it was proved that one of the fundamental units in the molecular structure is the aldobiuronic acid, 6-β-*D*-glucuronosido-*D*-galactose (IV). It was known also that there were present numerous arabinose residues which could be readily split off



One of the possible formulæ for Gum Arabic (F. Smith).

G = *D*-galactose residue; G* = *D*-glucuronic acid residue; R = *L*-rhamnose residue, or *L*-arabofuranose residue, or 3-β-*D*-galactosido-*L*-arabofuranose residue.

leaving a stable polysaccharide of less complicated structure. By examination of the hydrolysis products obtained respectively from the fully methylated derivatives of the original gum and of the degraded gum much information was obtained concerning the mode of linkage of the various residues. Further knowledge of the order in which certain of these residues were combined together was obtained by the isolation on partial hydrolysis of the two disaccharides, 3-β-*D*-galactosido-*L*-arabinose and 3-*D*-galactosido-*D*-galactose (V). By combining the various pieces of information it then became possible to advance a possible formula for the molecule of gum arabic and this noteworthy achievement by F. Smith is summarised in his formula for gum arabic.

Our review commenced with the allocation of ring structures to the simple sugars and methylglycosides and it has led us by way of the di- and tri-saccharides to the much more complex problems of polysaccharide chemistry which chemists are investigating, not unhopefully, to-day. But the carbohydrate field is so vast and is expanding so rapidly that only the barest outlines of recent developments could be sketched. I realise only too well that many matters of importance have had to be omitted but I trust that what I have been able to put before you will suffice to show that, despite all that has been achieved, innumerable problems of vital interest both to the chemist and to the biologist still remain for solution.

References

It is not practicable in this short review to cite individual references for all the work considered but the reader can readily obtain these from W. W. Pigman and R. M. Goepp's *Chemistry of the Carbohydrates* (New York, 1948) or from E. G. V. Percival's *Structural Carbohydrate Chemistry* (London, 1950).

STONY METEORITES

Address by

DR. W. CAMPBELL SMITH, C.B.E., M.C.

PRESIDENT OF SECTION C

THE scientific study of meteorites did not begin until quite at the end of the eighteenth century. The Ancients had stories of the fall of stones from the sky and some such stones were enshrined as objects of reverence. Except for the black stone of the Kaaba at Mecca all these have disappeared. For nearly 1,800 years stories of the fall of stones from heaven were everywhere discredited. As recently as 1772 a stone seen to fall at Lucé in France was reported by Lavoisier and two other Academicians to be an ordinary stone struck by lightning. In 1794 Chladni collected evidence to prove, and published his belief, that several masses of iron found on the earth had come to us from outer space. Chladni's announcement was followed by a shower of stones at Siena in Tuscany, June 16, 1794, at first attributed to Vesuvius or to the clouds accompanying its great eruption 18 hours previously. In the following year a 56 lb. stone was seen to fall at Wold Cottage, near Scarborough on December 13 and was exhibited to the public at a small shop in Piccadilly in 1796. Many stones were seen to fall after the impressive appearance of a ball of fire at Krakhut near Benares on December 19, 1798. These meteorites were all reported on as to their mineral composition by Count de Bournon, a mineralogist who had come to England during the French Revolution, and a report together with descriptions of the stones of Lucé, Ensisheim and Tabor was presented to the Royal Society on February 25, 1802, by Edward Howard. A further fall of stones took place at L'Aigle, Orne, France on April 26, 1803, and was exhaustively reported on by the physicist Biot. This may be said to have finally convinced the scientists of the early years of the nineteenth century that it is a fact that stones from outer space really do fall upon the earth's surface.

Such excessively rare objects as meteorites were soon eagerly sought after for the national collections in the big museums and it was around the collections in Vienna, Paris, Berlin and London that early research work in Europe centred.

The early studies of the mineral composition of stony meteorites depended on chemical analyses and on such information as could be acquired as to density, cleavage angles, and hardness of the several mineral components. With immense patience some investigators even extracted crystals which showed measurable faces and measured them to obtain confirmation of their identifications.

As early as 1861 (four years after Sorby had laid the foundation of microscopical petrography) Neville Story-Maskelyne had fitted a revolving stage and an eye-piece goniometer to a Powell and Leyland binocular microscope provided with polarising prisms and was using it to examine meteorites in thin sections. The sections are thick by our standards, and when we remember that in the British Museum in those early days daylight was the only source of illumination allowed, we must admire his discoveries in this field. In 1867 a chemical laboratory was fitted up in a house outside the museum and here Walter Flight was installed. Together Maskelyne and Flight produced a series of papers on meteorites which were to set a high standard for the future. Other early workers in the field were Brezina and Tschermak (in Vienna), Gustav Rose and Rammelsberg (in Berlin), G. A. Daubrée and Stanislas Meunier (in Paris), and J. Lawrence Smith and C. U. Shepard (in the U.S.A.). Of these Tschermak seems to have made most use

of the polarising microscope and he published in 1885 an admirably illustrated account of his observations. Rapidly the polarisation microscope was brought into use, but the chemical analysis of meteorites and of their component minerals has always held an important place in meteorite studies. The chemical analyses present considerable difficulties owing to the need for separate analyses of the metallic and stony portions. Both L. Fletcher and G. T. Prior worked to a high degree of accuracy and the latter is responsible for the demonstration of a ' law ' relating the iron-content of the silicate minerals to the iron-nickel ratio in the metallic portion of the stony meteorites. To these earlier workers the immersion method of determining the refractive index of minerals was unknown, and the measurement of optic axial angles by methods other than a direct one involving rotation in the field of a conoscope was at any rate little attempted. Unfortunately most of the chief contributors to the description of meteorites even in this century clung to the thin section study as their only means of identifying the minerals (aided of course by detailed chemical analyses). It is only in recent years in the United States and in South Africa that workers have made full use of the immersion technique and that refractive indices have been determined for pyroxenes, olivines and feldspars in meteorites. Fabric analysis has been used in the study of one fall and X-ray methods have been used only in one instance known to me. The result is that when one comes to make a study of the mineral composition of stony meteorites as a whole one finds oneself almost wholly dependent on chemical analyses and rather sparse information on twinning, extinction angles, and inclusions as observed in thin sections. Accurate optical data are largely lacking. The occasional, or one might perhaps say frequent, uncertainty in descriptions of minerals in thin sections is no reflection on the skill of the observers ; it is difficult to get thin sections from meteorites, and the mineral constituents are often quite turbid with inclusions or the fragments very small and confused. In quite recent times research on meteorites has moved on to study the isotopic composition and the quantitative distribution of the elements in meteorites and to radiochemical methods for the determination of the time since their final consolidation. With the results of this work I shall not deal in this address. I propose to confine my remarks to the information already derived from the chemical and petrographic study of the stony meteorites and perhaps to show where modern optical and X-ray methods can increase our knowledge and help to supply reliable data. On the knowledge so obtained we can hope to reconstruct the history of meteorite formation and perhaps throw some light thereby on the history of our earth and learn more of the deeper layers of its crust.

The main mineral components with which we have to deal in meteorites are few and simple. Leaving out a few rare minerals and some doubtful identifications they are:—Various alloys of iron with nickel, troilite (iron sulphide), carbon (including graphite and diamond), free silica as tridymite (asmanite) and quartz ; the silicates, olivine, various pyroxenes, and plagioclase feldspar ; the phosphates, apatite and merrillite ; and the oxides, magnetite and chromite.

The rarities to which especial interest attaches in the stony meteorites are, besides merrillite : maskelynite, anisotropic re-fused plagioclase ; oldhamite (CaS), osbornite (TiN) and, in a few rare carbonaceous stones, hydrocarbons. Some of these rare minerals are known only in meteorites. They may provide important clues to the conditions under which the meteorites were formed.

The main classes of meteorites are four :

1. Irons, consisting chiefly of nickel-iron.
2. Stony irons ; nickel-iron in large amount with some silicate minerals.
3. and 4. Stones (Aerolites). There are two groups : the Chondrites with nickel-iron, and the Achondrites with no nickel-iron or only very small amounts in exceptional stones.

Most of the meteorites that have been seen to fall are stony meteorites. Most of those found by chance (and not seen to fall) are irons. These are known from about 400 sites. Altogether some 1,300 authentic meteorite falls are known, of

which number about 600 have been seen to fall and most of these are stones. Writing in 1940 Max H. Hey reported that at that date of 599 meteorites seen to fall (and recovered) only 45 are irons and stony irons.

The stony meteorites called *chondrites* are characterised by the presence of small spherical bodies, called *chondri* or *chondrules*, consisting of one or more of the members of the pyroxene minerals, or of olivine and glass, or of olivine and pyroxene. All the chondrites contain nickel-iron though it falls to a very small proportion in some types. They constitute some 90 per cent. of the known stony meteorites. The other class, the *achondrites*, includes all those stony meteorites which do not contain these peculiar chondrules. The division between the two groups is not a very sharp one and it is not unusual to find that there are differences of opinion as to whether certain meteorites are to be classed with the chondrites or not. Although much the smaller group the achondrites afford the better material for study. Much of the material in the confused groundmass of chondrites is almost impossible to identify, but the achondrites present minerals in recognisable crystals and fragments. Moreover some parts of many of the achondrites resemble, though faintly, some kinds of terrestrial rocks. The degree of such resemblance has been greatly exaggerated in some of the literature on meteorites, but, such as it is, it affords us some clues to the way in which the stony meteorites may have been formed.

It is important at this point to take note that neither the meteoritic stones themselves nor all of their constituent minerals are in the condition in which they were first formed. Many of the achondrites, are intensely brecciated and show also effects of re-heating, sometimes repeated. Both mechanical and thermal changes have profoundly affected the constitution of the minerals and the texture of the meteorites.

It is convenient to consider first those achondrites, grouped by G. T. Prior under the name *aubrite*, and represented by seven falls of which the stones consist of 84 to 93 per cent. of nearly pure enstatite ($MgSiO_3$). The group includes Bishopville which provided the enstatite, then the purest known, for the classic work by E. T. Allen, F. E. Wright and J. K. Clement in 1906 on 'Minerals of the Composition $MgSiO_3$; . . .' Here also probably belong stones of the remarkable shower which fell on February 18, 1948, over a considerable area in Norton County, Kansas, and Furnas County, Nebraska. In Norton County alone over a thousand stones were found, one weighing 131½ lb. Six months after the fall a single stone was found measuring 42 × 39 × 36 inches estimated to weigh 1 metric ton, the largest known stony meteorite. The pyroxene is reported as enstatite with the optical properties of nearly pure $MgSiO_3$. (No chemical analysis is yet available.) Another remarkable stone of this group is that which fell at Bustee in the United Provinces, India, in 1852, and was described by Maskelyne and Flight in 1870. It was the first meteorite to be studied by Maskelyne by means of thin sections and a polarising microscope, and also the first meteorite to which X-ray methods were applied in this country for the identification of its constituent minerals. This stone, consisting mainly of pure enstatite, contains a nodule some 4 cm. across consisting of an 'enstatite-diopside' in which are embedded nodules of oldhamite (CaS) containing minute golden-yellow octahedra of the mineral named by Maskelyne, osbornite. In 1941 F. A. Bannister (applying X-ray methods) identified this osbornite with titanium nitride (TiN). As titanium 'cyanonitride' this substance is known in slags and bears from blast furnaces and in some pig-iron and ferro-manganese. Its melting temperature is 3,200° C., and it is readily converted to rutile at temperatures certainly below 1,000° C. in the presence of oxygen.

On the discovery of oldhamite Maskelyne wrote as follows :

> 'The presence of such a sulphide in a meteorite shows that the conditions under which the ingredients of the rock took their present form are unlike those met with in our globe; water and oxygen must have alike been absent. The existence of iron in a state of minute division, as often found in meteorites, leads to a similar conclusion.

But if the conditions necessary for the formation of pure calcium sulphide be borne in mind, the evidence imported into this inquiry by the Busti aerolite seems further to point to the presence of a reducing agent during the formation of its constituent minerals ; whilst the crystalline structure of the oldhamite and of the mineral next described (osbornite) must certainly have been the result of fusion at an enormously high temperature. The detection of hydrogen in meteoric iron by Graham, and more recently by other observers, tends to confirm the probability of the presence of such a reducing agent.'

A third aubrite, quite unique, in this case on account of its texture, is a rather weathered stone found in 1936 at Shallowater in Texas. It consists of a firm, reticulated aggregate of large (up to 4·5 cm. long) bladed crystals of colourless and transparent enstatite. The associated minerals are forsterite (5 per cent.), oligoclase (2 per cent.), and small patches of a nickel-poor nickel-iron (9 per cent.) and troilite (1½ per cent.). The analysis of the enstatite shows only 0·38 per cent. FeO and 0·32 per cent. CaO. All the other aubrites are intensely brecciated, though large crystals, 10 × 7·5 × 6 cm., are recorded in one. It may be that they had originally the same texture of large, bladed crystals as Shallowater and we are presented with the problem of explaining under what conditions such masses of large enstatite crystals could have formed.

Passing on now to a rather rapid survey of the remaining achondrites we find that they have been divided by Prior into two main groups ; (*a*) a calcium-poor group, to which the enstatite-achondrites just described belong, and (*b*) a group in which calcium silicate and anorthite both become more important. In the calcium-poor group, in addition to the pure enstatite meteorites, the mineral assemblages are : — olivine — ' clinobronzite,' hypersthene—olivine, hypersthene and/or clinoenstatite, and magnesian olivine (hyalosiderite). In the group richer in calcium the assemblages are :—bronzite — pigeonite — bytownite, pigeonite—bytownite, diopside — hortonolite, and one solitary lime-rich aluminous pyroxene. Let us look at each of these assemblages in a little more detail. In the lime-poor group the olivine—' clinobronzite ' assemblage is represented by three meteorites for the minerals of which we have little in the way of optical data. The olivine makes up to 62 to 67 per cent. and has an MgO : FeO ratio (later briefly referred to as *m*) of from 4 to 5. The pyroxene, referred by Prior to his ' clinobronzite ' and by Wahl to ' diopside-enstatite,' has *m* from 8 to 12. The texture is crystalline-granular and nickel-iron (5·5 to 8·5 per cent.) occurs as narrow films separating the grains. Novo-Urei and Goalpara are the two meteorites of this group that have been described. Goalpara is rather well known as an example of deep pittings radially arranged formed on the front side of the meteorite during its flight.

The hypersthene-olivine-achondrites of which seven or eight are known, are crystalline-granular aggregates with pyroxene about 33 per cent. and MgO : FeO for the combined silicates about 2·5. The meteorite which fell in 1871 at Roda, Huesca, Spain, is rather different. It is intensely brecciated. Olivine makes 22·38 per cent. and has *m* about 2·5, while the hypersthene is more magnesian with *m* near 3·5. The analysis shows 10 per cent. plagioclase, calculated as labradorite. ' Hypersthene '-achondrites (without olivine) are represented by five falls one of which was a shower from which twenty-seven stones were recovered near Johnstown, Colorado, in 1924. The total weight recovered was about 40 kilograms and of this over half (23·5 kg.) was made up by a single stone. This meteorite was well described by E. O. Hovey, G. P. Merrill, and E. V. Shannon. The latter identified the pyroxene as clinoenstatite, and he also recognised clinoenstatite in two other falls described in 1870 and 1871 by Maskelyne and Flight, who had referred the pyroxene to ' bronzite.' The MgO : FeO ratio in this group ranges from 2 to about 3·5. A little plagioclase occurs (bytownite in Johnstown), a little nickel-iron was recorded in Manegaon, and tridymite is recorded by Tschermak in two of the falls. With one exception stones of this group are brecciated and consist of splinters of pyroxene in a base

of its own debris. The one unbrecciated stone of this group is from Tatahouine, south Tunisia. The pyroxene is described by Lacroix as hypersthene and its composition is calculated as $3MgSiO_3 . FeSiO_3$.

The purely olivine-achondrites are represented by at most two meteorites. The only one described is the one which fell at Chassigny, Haute Marne, France, on October 3, 1815, of which about 4 kilograms of fragments were recovered. It was analysed by A. A. Damour in 1862 who showed that it consisted of olivine of composition near $2Mg_2SiO_4 . Fe_2SiO_4$. The optical properties of the mineral have not been determined.

This brings us to the end of our survey of the lime-poor group of achondrites. We shall meet with some of the same mineral assemblages among the chondritic meteorites.

In the lime-rich achondrites olivine occurs only once, as hortonolite, $Mg_2SiO_4 . 2FeSiO_4$, associated with a diopside and some interstitial plagioclase, in the meteorite Nakhla, named from El Nakhla el Baharia, east of Alexandria, where a shower of stones fell on June 28, 1911. Forty stones weighing together about 10 kilograms were recovered. They consist of a granular aggregate of diopside (76·7 per cent.) and hortonolite (13·25 per cent.) with interstitial feldspar, oligoclase-andesine (6·76 per cent.). The meteorite was thoroughly described by G. T. Prior and the optical properties of the minerals were determined. No other meteorite of this kind has been described.

One other unique kind of meteorite belongs to the lime-rich achondrites. This is a stone of 1·5 kilograms that fell at Angra dos Reis, Rio de Janeiro, in 1869. It was investigated by Tschermak 20 years later and was found to consist as to 98 per cent. of a purple, pleochroic, lime-rich aluminous pyroxene. Wahl re-examined the pyroxene in 1906, finding about 2 per cent. TiO_2 and comparing it to titan-augite. No other meteorite of this kind has been found nor has a similar pyroxene been observed in other meteorites.

The remaining members of the calcium-rich achondrites are very important. They are represented by about twenty-five falls. These all consist essentially of pyroxene and plagioclase, and nearly all contain large patches or fragments in which the texture resembles the ophitic texture of some dolerites and basalts. They show more resemblance to terrestrial rocks than any other meteorites, and it is on this account that much attention has been focused on them and several of them have been the subject of thorough investigations. They comprise two groups, the *eucrites*, so called from their resemblance to the anorthite-gabbros of the same name, and *howardites* which are now usually regarded as more brecciated eucrites. Ten eucrites are known and the total weight of these meteorites preserved in collections is about 142 kilograms (about 2·8 cwt.) so that for a meteorite the sample is adequate and they may be regarded as a rather wide-spread type. In bulk chemical composition they are rather uniform and not very widely different from, for instance, a Sudbury norite. With one exception the eucrites are much brecciated. Angular fragments consisting of pyroxene and plagioclase with usually some approach to ophitic texture are set in a firm base formed of the compacted debris of the same material. The plagioclase is bytownite-anorthite ranging in anorthite-content, according to H. Michel, from An_{84} to An_{91}. The ratio of plagioclase to pyroxene calculated from the bulk analyses is about 1 : 2. The pyroxenes are mainly monoclinic with some orthorhombic pyroxene recorded in at least two well-investigated stones of this type. The dominant pyroxene has been shown by Wahl (1906) to have a small optic axial angle and to belong to his enstatite-augite series. They are pyroxenes relatively poor in lime. In bulk composition they range from $CaSiO_3 . 2MgSiO_3 . 3FeSiO_3$ to $CaSiO_3 . 4MgSiO_3 . 4FeSiO_3$. According to modern pyroxene nomenclature this dominant pyroxene is referable to pigeonite within the somewhat restricted limits imposed by H. H. Hess (1941). Recent work by S. H. Haughton and F. C. Partridge on the eucrite which fell in 1936 at Macibini, Zululand, gives complete optical data for the pyroxene and feldspar, and the relative proportions of these two minerals based on micrometric measurements. The authors find two kinds of pyroxene, one of which is pigeonite, and they give reasons

for supposing that some of the CaO and Al_2O_3 which appears in the norm as feldspar really belongs to the pyroxene. The measured sections show the ratio pyroxene to feldspar about 4 : 1, twice the average calculated for eucrites by R. Brauns. A very thorough study has also been made of a eucrite which fell in 1913 at Carthage, Moore County, North Carolina, described in 1936 by E. P. Henderson and H. T. Davis, and further described in 1949 by H. H. Hess and E. P. Henderson. The feldspar here is bytownite and constitutes 40 to 50 per cent. of the stone, an unusually high percentage for feldspar in eucrites. Four pyroxenes are identified and complete optical data have been obtained. These consist of pigeonite (77 per cent.), augite exsolution lamellae (12 per cent.), and hypersthene (10 per cent.), with exsolution lamellae of clinopyroxene (1 per cent.). There is a little tridymite present and among other inclusions are minute blebs, possibly of nickel-iron. The texture of this meteorite is unlike that of the other eucrites as it is not ophitic nor is it brecciated but has a holocrystalline granular texture.

Lastly among these recent researches mention may be made of the description by W. F. Foshag of stones of the remarkable shower which fell near Pasamonte in New Mexico in 1933. This meteorite is classed as a howardite containing, along with fragments of ophitic texture, many fragments of different aspect consisting mainly of pyroxene, which is again pigeonitic. Orthorhombic pyroxene referred to ' bronzite ' has been recorded from some of the other howardites of which eight or nine are known.

Ignoring for the moment the obvious effects of the brecciation and the less obvious effects of reheating on the pyroxenes and feldspars, one can recognise in the ophitic-textured eucrites the products of direct crystallisation from a magma, as ophitic dolerites among terrestrial rocks are supposed to be. One cannot however, so interpret the crystalline granular aggregates of diopside—hortonolite, olivine, hypersthene, olivine — clinobronzite, and enstatite. These one must suppose are the result of crystal settling in a differentiating magma. The settling of slowly crystallising minerals on the floor of a magma chamber is regarded by Hess and Henderson as the cause of the granular texture of the Moore County eucrite mentioned above. Fabric analysis of the meteorite indicates primary layering such as might be produced by settling.

At the magnesian end of the achondrites a problem is presented by the Shallowater meteorite described by W. F. Foshag in 1940. Foshag finds it difficult to accept the mass of long (up to 4·5 cm.) bladed crystals as due to local accumulation of enstatite crystallising from a magma of different composition, and yet the known equilibrium relations in the system $MgO–FeO–SiO_2$ preclude the direct crystallisation of orthorhombic pyroxene from a melt of its own composition; clinopyroxenes should form first.

However this may be, a settling of crystals from a differentiating magma explains the texture of some of the achondrites, and it is important to note that this process requires not only bodies of magma of considerable size but a gravity field sufficient to make settling of crystals in the magma at all possible. Both requisites imply that these meteorites formed within a body comparable in size with our earth. There is no evidence as to the order in which the mineral assemblages of the several kinds of achondrites separated from the magma and very little evidence of the sequence of crystallisation in such meteorites as contain both olivines and pyroxenes. It may be noted, however, that the order of crystallisation expected from a magma of basaltic composition is from magnesium rich at the beginning towards the more ferriferous olivines and pyroxenes at the end ; that is to say one would expect a decreasing MgO : FeO ratio in the silicate minerals as differentiation proceeds. This is a point to which further reference will be made.

Leaving the rather dim picture of the meteorites as they were when first crystallised we must try and trace some part of what Paneth has referred to as their ' long and rather wild, geological history.' After crystallisation most of the achondrites seem to have suffered intense brecciation. This is seen at its most extreme in the pure enstatite-meteorites and in the hypersthene-achondrites, howardites and eucrites. It is not shown by the olivine-

rich types, nor in the lime-rich pyroxene stones Nakhla and Angra dos Reis. If it may be assumed that all the meteorites suffered the same kind of stresses it appears that olivine ' could take it ' and pyroxene could not. In the brecciated stones the matrix consists of compacted debris of the same material as the fragments ; eucrite in the eucrites, enstatite in the aubrites, and so on. There is nothing to suggest that the larger fragments have been driven very far apart. Other evidence of strain, presumably preceding brecciation, is given by bending and faulting of twin lamellæ in the plagioclase of some eucrites. According to F. Berwerth, who described such effects in the Peramiho eucrite, it is quite general in the feldspars and more rarely seen in the pyroxenes. Some partings and twin lamellæ parallel to the basal pinacoid may also be referable to pressure, as O. Mügge demonstrated experimentally for diopside.

Evidence of reheating after the brecciation of the primary crystal aggregates has been recognised in many of the achondrites. Its effects are shown both by the plagioclase and the pyroxenes. In many of the eucrites authors have described and attributed to heating effects the partial or complete conversion of the brown pigeonite to granules of a pale yellowish pyroxene. If optical data were available for these pyroxene grains interesting deductions could be made as to the probable temperatures attained. Lacroix has described in the Tatahouine meteorite the conversion of brecciated crystals of hypersthene to a crystalline mosaic and attributes it to reheating with consequent production of clinopyroxene. The requisite temperature would be, at normal pressure, about 1,080° C. A similar conversion of angular fragments of orthopyroxene to a crystalline granular aggregate is very pronounced in the hypersthene-olivine-achondrites Caratash and Manbhoom.

Actual fusion of eucritic fragments resulting in the production of dark glass is observed in the howardite, Luotolax, both by Wahl and by Lacroix. Some useful data might be derived by studying the relative effects of reheating on pyroxenes and feldspars in different eucrites and howardites. There is evidently a delicate balance and the compositions of the minerals concerned need accurate determination. Wahl observes pyroxene fusing before the plagioclase in the howardite, Luotolax, and in the Juvinas eucrite ; on the other hand in the eucrite, Peramiho, Berwerth notes that the heating caused fusion in the plagioclase rather than in the pyroxene.

Reheating is also held to be the cause of the development in the Shergotty meteorite of the mineral maskelynite. which is regarded as a plagioclase heated to softening point and converted into an isotropic pseudomorph retaining the form of the original feldspar. In this case the maskelynite composition calculated from the bulk analysis is near $Ab_{45}An_{55}$ for which the melting temperature, given by H. Michel, is something over 1,180° C. The pyroxene, which shows no sign of fusion, is slightly poorer in $FeSiO_3$ and richer in $CaSiO_3$ than the pyroxenes of most eucrites and therefore probably has a higher melting temperature. Shergotty is represented by a single stone, weighing when found about 11 lb. It was quite unique among meteorites until 1929 when a shower of many stones of the same kind fell at Padvarninkai in Lithuania. This should afford adequate material for a fresh study of maskelynite, a mineral quite unknown outside meteorites. Magnetite, extremely rare in meteorites, except in the outer fused crust, is recorded in both these stones, and implies an unusually high oxygen content compared to most other meteorites. Reheating to not much above 1,090° C., the conversion point of a particular hypersthene to pigeonite, was a late phase in the history of the Moore County eucrite as interpreted by Henderson and Davis in 1936. Hess and Henderson, however, in 1949, having revised the refractive indices and pyroxene identifications, abandoned this interpretation. They concluded that after complete crystallisation the pigeonite was held at a temperature above its conversion point (about 1,065° C.) in the deeper parts of the planetary body in which the meteorite formed, and that thereafter there was ' a catastrophic break up of the planet with great speed and sudden cooling ' so that the tridymite did not invert to quartz nor the pigeonite to hypersthene.

Many authors have discussed the conditions for formation and survival of pigeonite in igneous rocks and I dare not venture into this difficult field, but if I understand Bowen and Schairer aright it is not necessary to make the deductions made by Hess and Henderson of high temperature crystallisation and extremely rapid cooling. It seems probable that the pigeonites have 'formed metastably at temperatures below their stability range.'

There is some evidence in the achondrites for more than one period of reheating, but evidence of any extensive brecciation later than the reheating seems to be lacking. There is, however, one character possessed by many meteorites both achondrites and chondrites which shows that they were subjected to considerable crushing. Many meteorites are traversed by black veins of glassy material sometimes containing particles of olivine. Among these veins there is sometimes developed slickensides comparable to that seen on fault-faces in terrestrial rocks. These black veins of meteorites have been much discussed, but it is generally accepted that they are comparable to the pseudotachylyte veins described by Shand and others from South Africa, India and Scotland. The veins are different in composition from and clearly earlier than the glassy crust of meteorites formed in their passage through the earth's atmosphere. Everything points to their formation by rock crushing in bodies of considerable size before their final disruption into small meteorites.

None of the changes so far described are attributable to the heating effects caused when the meteorite entered the earth's atmosphere. These latter are restricted to the 'burning away' of the outer part of the meteorite, disruption with explosive violence, the formation of certain surface features and, last of all, of a thin glassy crust. The heat produced at the surface penetrated but a little way into the interior of the stones. All the earlier brecciation and reheating, and the formation of the 'pseudotachylyte' black veins may have been produced before the meteorites left their parent body. But there may have been more than one period of breaking-up and we must be prepared to consider as a possible cause both of brecciation and reheating, violent changes of temperature due to close passage to the sun of smaller masses than the original planetary body, to which cause has been attributed rather successfully some characters of the iron-meteorites.

When we leave the achondrites and turn our attention to the 90 per cent. or so of the stony meteorites called chondrites we come face to face with mineral associations and structures quite unknown among terrestrial rocks.

The chondrites are all characterised by the presence of the small spherical bodies known as chondrules and they all contain nickel-iron albeit sometimes in quite small amount.

The nickel-iron varies not only in amount but in the ratio of Fe to Ni from one chondrite to another. In 1916 G. T. Prior showed that, as regards a great number of chondrites for which reliable analyses were available, it could be established that 'the less the amount of nickel-iron in chondritic stones, the richer it is in nickel and the richer in iron are the magnesium silicates.'

The mode of formation of chondrites and their crystallisation and the reason for the relationship of the MgO : FeO ratio in the silicates to the Fe : Ni ratio in the nickel-iron are two of the outstanding unsolved problems presented by meteorites. Both may be due to conditions physical and chemical not realised within a planetary body at all but belonging to a later phase in the history of meteorites.

Prior's interpretation of the relationship he had discovered between the MgO : FeO and Fe : Ni ratios was that progressive oxidation of the nickel-iron, by which nickel was not affected, left the metallic portion enriched in nickel while the oxidised iron entered into the magnesium silicates developing first olivine and bronzite from enstatite, and thereafter increasingly iron-rich olivines and pyroxenes.

This was a development of a suggestion made by Wahl in 1910 that a stony iron containing metasilicate (pyroxene) and olivine could by oxidation be made to yield a pallasite consisting of olivine alone with iron relatively less abundant than in the unoxidised mixture.

It is of course a simple step to the idea

that the reverse process, loss of oxygen (reduction), could, starting from a pallasite (olivine alone in nickel-iron), produce a mixture of olivine and pyroxene with relatively more nickel-iron and that more iron-rich. Either way Prior's ' law,' as it is sometimes called, holds.

Following up his idea Prior divided the chondrites into four groups (later reduced to three), based on (1) the ratio, (*m*) of MgO : FeO in the silicates, and (2) the ratio of the percentage weight of Fe : Ni (*n*) in the metallic portion of the stones. In the first group (1) the silicate is enstatite and oldhamite, the calcium sulphide, is present as an accessory constituent. Nickel-iron constitutes 18 to 25 per cent. and the ratio Fe : Ni (*n*) ranges from 10 to 13. The iron sulphide, troilite is present (7 to 8 per cent.) and there is a little graphite. The other groups in order, are : (2) bronzite-olivine-chondrites with MgO : FeO about 5 and the nickel-iron about 10 per cent. with *n* lower than in the enstatite-chondrites ; and (3 and 4) hypersthene - olivine - chondrites with *m* from about 4 to 2½ and nickel-iron in rather small amount but richer in nickel (*n* 7 to 3).

In regard to the ferromagnesian minerals they contain the chondrites correspond to only the first three groups of the achondrites, i.e. those consisting of pure enstatite, olivine — clinobronzite, and hypersthene — olivine assemblages. Neither the purely hypersthene-, nor the olivine-achondrites, nor any of the relatively calcium-rich achondrites (eucrites, etc.) are represented among chondritic meteorites.

It may also be remarked that chondrules never occur where nickel-iron is entirely absent, though admittedly the nickel-iron content falls as low as 4½, or even 1½ per cent. if the Jelica stone be included, as it probably should be. On the other hand, the presence of nickel-iron in stony meteorites does not ensure the appearance of chondrules. There are a few achondrites, within the composition range of the chondrites, which do contain nickel-iron and yet are not chondritic. The most notable instance is the unique, enstatite stone of Shallowater already referred to, in which 9 per cent. nickel-iron is reported.[1]

The chondrules themselves are of a great many different types. Those which are outstandingly different from anything known in terrestrial rocks are the so-called radiating enstatite chondrules. These consist of radiating prisms (or plates) of orthorhombic pyroxene. At their most perfect they are truly spherical, sharply bounded, and the radiating prisms, as they appear in thin sections, spring from a point not central but eccentric.

In addition to these, Merrill records cryptocrystalline and glassy chondrules which are rather presumed than known to have the composition of ' enstatite.' Others consist entirely of clinopyroxene (enstatite-augite of Wahl), or of olivine, or, a single record, of anorthite. Those containing more than one mineral consist of olivine and orthopyroxene, olivine and ' glass,' and, rarely, olivine with plagioclase and troilite. A rounded outline is common to them all and this is often truly circular in section especially in chondrules of the radiating orthopyroxene and glassy or cryptocrystalline types. The olivine chondrules may be porphyritic or granular, consisting of several crystals, or they may consist of a single crystal or parts of a crystal in optical continuity. A remarkable variety consists of alternate layers of olivine and dark interstitial material variously observed to be glass or an aggregate of bronzite fibres or of olivine and bronzite. These are the so-called barred forms. They and many other olivine chondrules often have a border of clear olivine, parts of which are sometimes in optical continuity with the bars in the interior. Similar narrow rims occur round enstatite chondrules too. A quite different kind of border in some chondrules is formed of nickel-iron and troilite.

The matrix of many of the typical chondrites is a fine-grained, compact to friable, aggregate of olivine and pyroxenes with varying amounts of interstitial nickel-iron and troilite through which the

[1] Teclu (1870) reported 8½ per cent. nickel-iron in the Goalpara achondrite, and 5½ per cent. is reported in Novo Urei with smaller amounts in Manbhoom, Bandong and Vavilovka. It is only fair to remark, however, that of these five, one, Novo Urei, was originally classed as a chondrite. Goalpara and Novo Urei are olivine-clinobronzite- and the other three are hypersthene-olivine-achondrites.

chondrules are scattered. These are the chondrites to which the perhaps somewhat misleading term ' tuffaceous ' has been applied. In some types the chondrules are not numerous ; in others the meteorite consists almost entirely of small very definite chondrules and their fragments in a dark matrix. Again in other types, the crystalline chondrites, the chondrules almost merge into the groundmass and their outlines are vague. Such is the case with the rather large, vaguely outlined chondrules of radiating enstatite with enmeshed nickel-iron in the enstatite-chondrites, all four of which are ' crystalline ' chondrites in Brezinas' classification.

Changes undergone by chondrites, after the formation of the chondrules, have produced fragmentation of the chondrules themselves and some reheating effects. The black colour of some chondrites is attributed to heating and has been imitated in the laboratory, but in some chondrules it is due to the presence of carbonaceous matter. A few chondrites actually contain hydrocarbons.

Lastly the black veins observed in some achondrites are very frequently seen in the chondritic stones.

Many views have been expressed as to the origin of chondrules. G. P. Merrill, reviewing the problem in 1920, came to the conclusion that the chondrules of glass and crypto-crystalline or radiating prismatic orthopyroxene—and only these—could be regarded as having crystallised from globules of liquid silicate. This was a view expressed by H. C. Sorby in 1877 and by Tschermak in 1882. The porphyritic and granular olivine chondrules and those consisting of single crystals he finds have rougher surfaces, sometimes quite spherical but grading into angular fragments with angles blunted or rounded. Such chondrules he considers must have crystallised from a melt of ' some considerable proportions ' and owe their present form to attrition while in a soft or plastic state. This idea of rounding by attrition was advanced by Tschermak in 1874 and by Daubrée in 1879. The original suggestion was that the (porphyritic and granular) chondrules became rounded under conditions such as might exist in the throat of a terrestrial volcano. It must also be noted that the view has been put forward, for instance, by Borgström, writing of the Shelburne meteorite (in 1904), that ' because chondrules of the same chemical composition have a different structure, they must have formed under different physical conditions. Since such a variety of conditions can not have existed in the narrow space in which the different structures now are met with, the chondrules must have accumulated after solidification.'

The idea that some meteorites are composite and that some of their component fragments have had a separate existence as small meteorites and have been gathered in by larger meteorites has frequently been suggested, and at least one definite instance of this, the Cumberland Falls, Kentucky, meteorite, has been described.

As regards the enstatite-chondrules, however, I do not think they can ever have been far removed from the meteorite in which they are now found. As far as the available chemical and optical information goes, it appears that the pyroxene of the radiating ' enstatite ' chondrules is not always enstatite, but that it is bronzite in the bronzite-chondrites and hypersthene in the hypersthene-chondrites. This is an important point which needs to be further investigated. If it is proved, the inference is that the radiating chondrules have crystallised from a melt of the composition of the chondrite in which they are now found, and that they are not ' foreign bodies.' As for the olivine chondrules they too appear to have compositions appropriate to their present surroundings. If they owe their rounded form to attrition as is supposed, they may have acquired that form without ever leaving the body, necessarily a large one, in which they finally crystallised.

The general characters of chondrites and chondrules as regards both structure and composition, which I have described, must be accounted for in any acceptable theory of their origin. The main points in order are these :

(1) The chondrites appear to be agglomerations of chondrules in a brecciated matrix and some, at least, of the chondrules can hardly be imagined to have crystallised in

spherical forms except as free globules outside the meteorite.

(2) Chondrules never occur when nickel-iron is entirely absent.

(3) They only occur with minerals of the hypersthene—olivine assemblage for which MgO : FeO is from $2\frac{1}{2}$ to 4, and with more magnesian assemblages.

(4) They obey Prior's 'law.'

At the time of writing I have no explanation to offer of (2) and (3) but they are in some way related. Chondrules are not found in the pure hypersthene-meteorites ($m = 2$ and less) : nor is nickel-iron, except in certain mesosiderites, which are composite meteorites. As to nickel-iron always being present in chondrites, this may be controlled by some equilibrium between nickel and iron and the process of oxidation or reduction which operates Prior's law. If iron is being oxidised and passing from metal to silicate something prevents the last of the iron from going. Pure iron-free nickel is not found in meteorites.

As regards Prior's law the relation he established implies that the nickel-iron and the ferromagnesian silicates co-existed in bodies of sufficient size, and enduring as such for a sufficient length of time, to allow interchange of iron to take place between the metal and the silicate portion. This may have taken place while both metal and silicate were liquid and immiscible. The interchange may have been made possible either by addition or by loss of oxygen.

Differentiation of the silicates and settling of successively more and more iron-rich silicates from a magma, which was sufficient to explain the achondrites, will not here suffice, for although it explains the decreasing MgO : FeO ratio in the silicates it leaves unexplained the corresponding loss of iron by the metallic portion.

Turning now to the chondrules, we find these crystallising from melts already adjusted in composition to the particular nickel-iron with which they are now associated but the enstatite and the olivine each under different conditions. The orthopyroxenes, enstatite and bronzite, crystallised from molten globules of silicate of their own composition : the porphyritic olivines crystallised more slowly in larger masses, which were thereafter broken up and rounded by attrition probably while still soft and plastic. And then the whole mass of chondrules is gathered together again into the brecciated mass of silicates and interstitial nickel-iron which now composes the matrix of the chondrites.

To account for these strange happenings the story of the chondrites may have been something like this :

Crystals of a magnesium silicate, perhaps enstatite as Prior supposed, settling into the nickel-poor nickel-iron layer of planetary body. Break-up of the planet starting large bodies torn from its deeper layers on their course as vast meteorites. Owing to the difference in the paths and therefore different heating histories or different opportunities of access to supplies of oxygen (or hydrogen ?) of these large masses, some had better opportunities for the interchange of iron between the metal and the silicate portions than others. This would result in the range of composition found among the chondrites. At some later period in their course, probably due to close proximity to the sun, intense heating and violent fusion (? boiling) is accompanied by ejection of globules of molten pyroxene and the fusion of other portions from which olivine slowly crystallised. When cooling was possible the globules of metasilicate crystallised rapidly retaining their globular form, and finally were gathered back into the mass from which they came, now a brecciated mass of fragments and rounded chondrules of various types, with, in its interstices, still molten nickel-iron. Such processes could be repeated more than once. Thereafter came, at later stages in the history of the chondrite, brecciation of the now solid chondrules, such reheating as has formed black chondrites and minor heating effects, and the development in some large masses of the late, black veins and slickensides. And lastly, a breaking up into smaller masses, arrival in the earth's atmosphere, burning away of the outer surface and the formation of the thin outer crust, landing at last, and, with good fortune, finding a home for many years and an honoured place in the famous collection of some great museum.

THE SCIENCE AND PRACTICE OF ENTOMOLOGY

Address by

DR. V. B. WIGGLESWORTH, F.R.S.

PRESIDENT OF SECTION D

ENTOMOLOGY in Great Britain remained for long enough the care of amateurs. A long line of distinguished naturalists whose chief interest lay with the insects runs through the nineteenth century. There was A. H. Haworth (1767–1833), a lawyer by profession, the founder of the first entomological societies in this country ; there was William Kirby (1759–1850), Fellow of Gonville and Caius College and rector of Barham in the county of Suffolk for fifty eight years, who, with William Spence (1783–1860), produced the standard work on insects that was to hold the field for half a century. There was George Newport (1803–54), physician and surgeon, who published a remarkable series of papers on the anatomy and physiology of insects. There was the lawyer's clerk and engraver John Curtis (1791–1862) and Miss Ormerod (1828–1901) who, in the second half of the century laid the foundations of agricultural entomology in Britain. There was Sir John Lubbock (Lord Avebury) (1834–1913), worthy of remembrance indeed, for it was he who sponsored the Early Closing Act and gave us our bank holidays. As a relaxation from a most active public life, Lubbock laid the groundwork of our knowledge of the sensory physiology of the Hymenoptera ; it was Lubbock who proved that bees distinguish colours and can be trained to associate particular colours with the presence of food ; that the vision of ants extends into the ultra-violet ; and who suggested, far ahead of his time, that ants find their way home by remembering the direction of the sun.

Professional entomologists there were, in the museums : W. E. Leach, J. F. Stephens and others at the British Museum from early in the century, Westwood at Oxford from 1861 ; David Sharp at Cambridge from 1885. But the tradition of the subject remained that of the naturalist and the collector. It was the private collector who built up systematic entomology in this country—and he still plays a valuable part. Implicit in all I shall say in this address is the need for sound taxonomy. We are unlikely ever to have enough taxonomic specialists in our museums to deal with all the multifarious groups of insects. We still need, and I think we shall always need, the competent amateur to fill the gap.

It was probably this tradition which led to the neglect of insects when the scientific teaching of zoology in our Universities was being established during the second half of the last century. It is true that in formulating his ' type ' system T. H. Huxley adopted the cockroach as one of his ' types '—but there the study of insects stopped, and there it has tended to remain.

INSECTS AS A MEDIUM FOR THE STUDY OF BIOLOGY.

This was singularly unfortunate, because the insects compose perhaps three-quarters of the known species of animals. They are exceedingly diverse in form and habit, readily available for study, and well suited to illustrate, often in the most vivid and accessible form, most of the problems with which the student of living things is concerned.

One by one the natural sciences have found among the insects ideal material for study. The laws of heredity in insects provide one of the main pillars which

support the science of genetics ; the role of the genes in the determination of sex was first deduced from a study of sex-linked inheritance in the Lepidoptera ; the insect has provided and is providing wonderful raw material for the organic chemist and for the student of fine structure on the frontiers between chemistry and physics ; insects have proved excellent material for the assay of vitamins and they can be expected to provide opportunities for the recognition and physiological study of new vitamins ; the phenomena of growth, metamorphosis, wound healing and regeneration can be seen in the clearest light in the comparatively simple epidermis of the insect. The study of neuro-muscular physiology in insects and other arthropods is contributing greatly to our knowledge of that subject. The aero-dynamics of insect flight are not only interesting in themselves but can illuminate the problems of animal flight in general. And for the mechanistic analysis of simple types of behaviour insects have provided more varied material than any other group. One would, of course, be guilty of a grave solecism in the hierarchy of learning if he were to stigmatize students of such matters as ' entomologists '—but the fact remains that zoologists, physiologists, physicists and chemists have turned to the insect with increasing frequency in recent years.

Last, but not least, the insects provide some of the best material for the ecologist. In his Presidential Address last year Professor Hardy was covering so vast a canvas that he could scarcely find place to mention that much of the most illuminating work on the balance of populations in nature is being carried out among the insects. Within this group the interactions of climate and food supply, predators, parasites and infectious diseases of all kinds provide one of the most fertile fields in which to cultivate zoology outside the laboratory. Indeed, this is where most of our current ideas on the subject have come from.

The Growth of Applied Entomology

There is another side to the picture. The period during which zoology became established in the Universities in this country was also the period of development of intensive and large scale agriculture and of great improvements in communications, with the consequent emergence and dissemination of insect pests. The change was heralded in the 1860's by the transformation of the harmless little Colorado beetle into a major pest of agriculture, sweeping across the American continent and, to-day, across Europe. The production of Paris green and lead arsenate as agricultural insecticides was the result. And since that day the pests have become ever more prominent and the insecticides and other methods of control more powerful.

During this same period, starting with the work of Patrick Manson on filariasis in 1878, came the discovery that insects or related arthropods are responsible for the transmission to man of nearly all the great epidemic fevers of the tropics : yellow fever and dengue, malaria, the relapsing fevers, typhus and all its varieties, bubonic plague, five day fever, sleeping sickness, kala azar and the rest. The same proved true of many diseases of livestock : trypanosomiasis in Africa, the tick borne fevers of cattle in South Africa and the United States, and many more.

The increasing adoption of plantation methods for growing crops in the tropics, and now the first beginnings of large scale food production for export in Africa, are fast creating conditions for more and more insect problems. The current instability of human society is leading to more attention being given to the possibilities of storing large reserves of food. Storage brings in its train the problem of controlling insect pests in stored produce. It was a great step forward when the millers in this country, ten years ago, became interested in controlling infestations of insects in grain. If, as seems likely, food is to be stored in the hot climates of Africa this problem is destined to thrust itself more and more upon the attention of Governments. The locust has been with us since the dawn of history ; but it can still bring famine to a whole community. Only improvement in communications and the development of food storage enables the importation of food to soften the blow. The importation into this country of infested hardwoods, and the widespread use of timber of poor quality

in which sapwood is abundant, have led to a great increase in those insects which destroy structural timbers; and their importance is likely to enlarge as new pests become distributed and as the world shortage of timber continues to make itself felt.

All these developments have led to an ever increasing recognition of the significance of insects in human economy. The insects have surely come into their own—in pure science and in applied.

Training and Outlook of the Entomologist

It is upon this present state of affairs that I want to reflect for a little while in this address. The use of insects for the study of the fundamental problems of physiology is a subject on which I have spoken elsewhere (1). Insects not only furnish ideal material for the detailed investigation of many of these problems, but by reason of their relatively small size they encourage the observer to think about the body as a whole and to reflect upon the unity of the organism—an important matter, of which the classical physiologist seems at times to be scarcely even aware.

I do not propose to say much about the insect as a medium for the study of biology in general—beyond this: I think there are still unbounded opportunities for exploitation. But these opportunities will only be developed to the full if we turn out *entomologists*; men who know the world of insects in the round; who are at once morphologists, taxonomists, physiologists and field naturalists. As the painters' dictum has it: if we take care of the shadows, the high lights will take care of themselves.

The majority of entomologists, professional entomologists that is, must be applied entomologists; and it is of applied entomology that I wish chiefly to speak.

In some branches of science man seems to be confronted with a fixed set of relations which successive generations of students are patiently uncovering. But the world of insects is like human society; it is in a constant state of flux. Of course, the student of insects is inspired by that same religious faith in the fixity of natural laws which is the sheet anchor of all scientists. But he is acutely conscious of the inconstancy of his material. Changes in nomenclature seem to be rendered interminable by the laws of priority, operating like the old Court of Chancery. But it is not only in this artificial science of synonymy that the insect world is inconstant. The fixity of species which disappeared in theory with the acceptance of the doctrine of evolution, is found to have disappeared in practice when the entomologist looks as closely into his insects as the applied entomologist is often obliged to do. The *Anopheles maculipennis* of the museum dipterist has been splintered to a dozen entities more or less sharply defined by differences in physiology or behaviour or in the morphology of their eggs or other stages. The student of the Aphids is trying to find some way of dealing with groupings which would surely be regarded as good species were it not impossible to find morphological differences between them. Indeed, the applied entomologist is often embarassed by the discovery of what must be so if evolution is in progress—that the species tails off into the geographical race, or into the biological race, where clear cut differences in physiology or mere statistical differences in behaviour exist in the absence of detectable morphological characteristics. And, apart from such major changes, species are continually changing in abundance and in distribution, in their choice of foodplants and in the amount of damage to human interests they bring about.

Some of these changes we see going on around us are no doubt the product of natural evolution outside the control of man; but very many of those with which the applied entomologist has to deal are the result of human activities—changes in agricultural practice which disturb the balance of populations, an unfortunate choice of crop rotations, or the abandonment of any rotation at all; or, may be, the introduction for other reasons of varieties of crops that prove highly susceptible to insect attack.

Applied Entomology and Research

If I am correct that the world of insects is in a state of flux, it follows that the entomologist must be constantly investigating.

Indeed, it is my contention that *applied entomology is research.* Once a problem in applied entomology is solved the preventive procedure can be incorporated into the practice of the industry in question and the entomologist is no longer required. That, of course, is an overstatement; despite the advances in preventive medicine we still need the general practitioner. Some procedures demand the special knowledge of the entomologist for their proper application; or their correct timing may depend on accurate identifications possible only for the trained entomologist. It has been found by Dr. Massee and his colleagues in Essex that in the presence of certain insect predators it may do more harm than good to carry out summer spraying in orchards (after June) against the Fruit Tree Red Spider Mite. Only an entomologist can recognise the predatory species among all the other insects present. The classic example is the infestation of field crops by Aphids. It takes more than the intelligent farmer to decide whether it is wise to apply insecticides or whether the infestation is about to collapse before parasites or infectious disease.

But I think the assertion is broadly true. The entomologist, in fact, is continually working himself out of his job. It is, however, a provision of nature that, as a result of man's activities, sometimes, it must be confessed, as a result of the activities of the entomologist himself, no sooner is one problem solved than another, often more difficult, arises for solution.

The entomologist, I repeat, must be an investigator. He must acquire and retain the research outlook, the inquiring mind, the determination to find out for himself by experiment. I have spoken already of the value of the insect as a medium for the study of physiology. Here I should like to enter a plea for the study of insect physiology for a different reason: for the reason that it forces the applied entomologist to look far more closely into his problem that he would otherwise do. Last year you listened to an admirable defence of ecology by Professor Hardy. I wish to detract no word from that. But again and again it has been my experience that the physiologist has been able to point out the key problems in the life of the insect in its natural environment and so to give a valuable lead to the ecologist who is concerned with the balance of natural populations. The physiologist who comes to take an interest in populations, who combines the aspirations of the zoologist within and without the laboratory, will in my view make the best ecologist. And, by the same token, some experience in physiological research will provide one of the best introductions to a career in applied entomology.

Interaction of Research and Practice

If those are the implications of my thesis so far as scientific education and outlook are concerned, what are the implications in the field of practice and administration? It is a truism that the applied entomologist, if he is to be effective, must come to regard himself, primarily, as a member of the industry which he serves. He must acquire the outlook and sympathies of the sanitarian, the farmer, the wharfinger or whatever it may be. But when that is done, the basic problem as I see it is how to keep the entomological investigator in close touch with the day to day problems of his industry without stultifying him as a research worker.

We are all familiar with the plea, made as a rule by those of limited acquaintance with both research and practice, that we should call a halt to all this research and concentrate on using existing knowledge. I have no wish to disparage the very great importance of the dissemination of knowledge (the British Association exists for that very purpose), but anyone with the smallest acquaintance with practical prolems knows that at every point practice is held up by lack of knowledge. The sense of frustration often comes from the fact that the applied entomologist who sees what we need to discover is so occupied with petty tasks that he has not the chance to fill the gap.

I have spoken of the value of physiological studies on insects in opening the eyes of the ecologist and applied entomologist. Of no less importance is the stimulus which the study of practical problems gives to the physiologist. The interest in insecticides, which became so widespread during the war, has been a potent factor in bringing

about the revolution in our knowledge of the insect cuticle which has taken place during the past ten years. The intensive study of the moisture and temperature relations of insects which has yielded results of great physiological interest was very largely stimulated by the applied entomologist. And he it was who started the study of population ecology in insects. But if these mutually beneficient influences are to be felt there must be an unbroken chain of research workers at all levels in sufficiently close touch with one another to appreciate each other's problems.

Administration

The administrative requirements will vary in the different branches of the service; but the keynote throughout should be the same: the safeguarding and encouragement of research. Anyone who has tried both, knows that administration is so immeasurably easier than research that it becomes the line of least resistance and that is why research needs encouragement.

Let us consider a few examples of what I mean, selected from the field of entomology in this country and empire, and reviewed in a spirit not of criticism but of detachment. Take a small example first. In my opinion an institution like the British Museum (Natural History), apart from its most important function of exposition, is primarily a centre of research in the classification of animals; and in the interests of this research the staff should be protected from trivial incursions by the existence of a screen of public relations officers who would provide the connecting link between the general public and the specialist—to the advantage of both.

The problem is a larger one in the applied field and it is encouraging that some of the latest recruits to the entomological service of this country seem to have shown the clearest grasp of the requirements. The Pest Infestation Laboratory built up by the Department of Scientific and Industrial Research during the past ten years, upon the foundations laid by Professor J. W. Munro, is a research laboratory. It covers the whole range of problems from such technical matters as the disinfestation of bulk grain in huge silos, or the devising of insect-proof wrappings for packages of foodstuffs, down to the physical and physiological factors concerned in the passage of toxic substances through the cuticle of the insect. Close touch with the practical world is maintained through the Infestation Branch of the Ministry of Agriculture, which deals with all the day to day matters concerning insects in produce and which in this way provides the research laboratory with the practical problems that are in most urgent need of investigation and with the industrial contacts that are necessary for their solution, but at the same time protects the investigators from unprofitable distraction.

The same desirable gradation is to be seen in the Anti-Locust Research Organisation—although this still rests on a temporary footing, with all the resultant uncertainties. The Anti-Locust Research Centre under the Colonial Office in London provides a clearing house for all information about the current activities of locusts and locust control organisations in all parts of the world. The various colonial governments have their own schemes of locust control. But the bulk of the research on all locust problems in the colonial empire ranging from the techniques for killing locust swarms, or studies in ecology which aim at preventing the emergence of swarms, down to research on the detailed physiology of the locust in all its varying phases, is administered and co-ordinated by the centre. The virtue of this scheme, which has yet to come to fruition, lies in the opportunities which it affords for many-sided contacts and for mutual stimulation by research workers at all levels.

These are fields in which advances in both science and practice have been rapid during recent years. The same cannot be said for our knowledge of infestations by insects in timber; and I think the reason is not far to seek. The entomology section of the Forest Products Research Laboratory, which is our chief centre for work on this subject, has no screen between it and the building industry. As a result we find the research workers so occupied in answering letters and giving advice, using knowledge that is admittedly inadequate, that although very creditable

advances have been made, the extension of knowledge by research has been severely handicapped. We are caught in a vicious circle which will be broken only when it becomes possible to insert a screen of advisers or inspectors, stationed in immediate contact with the research laboratory (responsible perhaps to the Ministry of Health) who would provide the research workers with the essential link with practical problems but would shield them from trivial interference and enable them to get on with their proper work of investigation.

Agricultural Entomology

By far the largest employer of entomologists in this country is the National Agricultural Advisory Service. Built up under the guidance of the late Sir John Fryer, the specialist advisory service for the farmer was taken over by the nation three years ago. The entomologists in the service, who in the past were associated with the various centres of teaching and research in agriculture throughout the country, were then reorganized as an entirely independent service. It is not difficult to appreciate the attractions of such a scheme from the point of view of administrative tidiness and convenience. But it will be no easy task after such an uprooting to build up a new tradition of first class research—research, not into those general principles with which the student in the Universities is commonly occupied, but research into the fluctuating insect problems that confront the grower in the locality in question ; research which differs in kind but should not differ in quality from that carried out in the institutions from which the service is now detached.

There is within the service a provision of County Officers who should become increasingly competent to deal with most of the day to day matters of entomological advice and so leave the provincial specialists with a substantial part of their time free for the investigation of those problems needing close study. Perhaps the first step will be to convince ourselves that the specialist branch of the advisory service, at any rate in entomology, must be concerned primarily with investigation ; that the entomologist cannot cope with the changing problems that arise in agriculture without research and experiment as a broad basis ; and that the final link in this chain of research must be forged by the specialist adviser. One thing is certain ; without this the service will not attract or retain (as it did in the past) entomologists who will be known by name and who can really contribute to the improvement of agricultural practice in this country.

Behind the advisory service will stand in due time the series of oddly named ' husbandry farms ' more specifically concerned with trials and experiments ; presumably the entomologist will sometimes play a part here. Then there is the series of crop research stations, some long established and others coming into being. And behind these again the Agricultural Research Council with its co-ordinating committees and conferences, its research units devoted to special subjects (I myself am associated with one of these dealing with insect physiology), and its special research grants to University Departments. The machinery is there ; it is highly flexible and is in the hands of independent scientists in the University Departments and elsewhere. If we do not make use of it in advancing the science and practice of entomology we have only ourselves to blame.

Entomology in the Colonies

The same problems arise when we consider entomology in the colonial empire ; although here are several new elements that demand consideration. There is so much that remains unknown in applied entomology in the tropics that the duty of the entomologist to pursue research is patent to all. But if we are going to get the best value for money spent on research we must aim at providing the right conditions. The entomologist in the tropics has adequate equipment ; laboratories are often more spacious than at home and assistants more plentiful. What he is apt to lack is the stimulus of emulation (the most potent factor in breeding research) the intellectual stimulus of scientific contacts, and access to libraries and centres of research.

The scientific services in the colonies differ from the administrative services fundamentally in these respects. It should therefore be recognised that the entomologist ought to put in a very substantial part of his time in this country (or in some suitable centre in Europe or in the Dominions) not on leave, but at work. The presence of a changing population of such men in our institutions at home to which they would be temporarily attached, could not fail to have a mutually stimulating effect.

The practice of a liaison of this kind would facilitate the exchange of staffs. The young, or not so young man taking up entomological research in the tropics should be able to feel that he will not lose touch with work at home, and that if he makes good there will be no insuperable obstacle to his transfer later to a post at home (and vice versa). From this point of view the service would be much more attractive if the terms of appointment and of pension were of a kind that would make possible transfers of this sort. That is one of the attractive features about the Colonial Research Service that is gradually coming into being. This is not a matter on which it is useful to express dogmatic opinions in a few words, but there is something to be said for treating other entomological appointments in the colonies in the same way.

Insecticides

But there must be many here who are asking themselves: Is all this harping upon entomological research really necessary? Is not the entomologist in fact outmoded by the discoveries of the organic chemist in the realm of insecticides? There is indeed a belief current to-day, a belief that is very naturally fostered by those whose material interests lie in that direction, that the future of insect control lies not with the entomologist but with the chemist.

Now, here let me add my meed of praise to those chemists in Switzerland, Great Britain and Germany who discovered respectively the insecticidal properties of DDT, Gammexane and the organo-phosphorus compounds, materials which have transformed the control of insect pests during the past ten years. This has been a remarkable achievement; and its impact is by no means expended, for more and more synthetic insecticides are produced as one manufacturer vies with another. Not only is this a remarkable achievement in itself, but it appeals to the human mind. The public loves the hospital, the doctor and the bottle of physic; while the advances in preventive medicine which have transformed our lives are scarcely noticed. So too it creates a greater impression on the mind to destroy an infestation of insects that can be seen, than by some simple change in practice to prevent any infestation from developing. Effective methods are known for preventing the breeding of house flies, methods based on simple biological principles. But I know of at least one institution which could not be interested in such methods but adopted instead the practice of spraying the walls with DDT. The house flies in that institution are now among the most DDT-resistant in the world.

It is indeed becoming increasingly evident that the use of more and more insecticides is creating insect problems as fast as it solves them. This is no new discovery. It has been realized for many years that in the orchards, for example, an ever increasing number and variety of washes were becoming necessary to secure clean fruit. A. D. Pickett (2) points out that in the orchards of Nova Scotia during the early part of this century two or three applications of spray applied with hand-operated pumps were sufficient to give fruit of good quality. To-day, with greatly improved equipment and far more potent chemicals six or even ten applications are required to produce crops reasonably free of insect damage. The familiar mussel scale *Lepidosaphes* has been known in these orchards for more than fifty years; but only in the past fifteen years has it become a pest—and then only in those orchards which have been most thoroughly treated with lime sulphur for the control of apple scab. That is because the Chalcid *Aphelinus mytilaspidis* and the predaceous mite *Hemisarcoptes malus* which normally hold the scale in check are killed by the sulphur. The same has happened with DDT which has produced

great outbreaks of the Woolly Aphis, the Red Spider Mite and other pests. We can, as Pickett points out, probably control any specific pest with which we have to deal—but the problem of pests in general gets worse. New machinery and new chemicals are developed, and we are carried from crisis to crisis, always hoping that the newest chemical or the newest technique will provide the final answer.

Where is this process going to end? Already the number of washes recommended in some parts of the world is reaching the limit of what it is economic to apply. We do not know what the end is going to be. Not even the most enthusiastic entomologist believes that chemical control in orchards can be dispensed with. What we need is more control of the chemists. We need more biological research to see just exactly what effect the chemical applications are having upon the insect populations as a whole.

The Future of Applied Entomology

We need not less but more entomological research. By all means let us use insecticides when we can do no better. But we should regard them, as C. B. Williams has emphasised more than once, as an admission of failure, to be replaced by the more subtle and more remunerative methods of biology as soon as these can be worked out.

Can this be done? Even in the twentieth century necessity is still the mother of invention, and when economy requires it will be done. It was the economic depression of the early thirties which provided the stimulus for the working out by G. C. Ramsay (3) in Assam of the cheap and highly effective means of malaria prevention by the shading of the breeding places of *Anopheles minimus*.

Not long ago we were told by politicians that the problem of veterinary trypanosomiasis in tropical Africa had been solved by the discovery of the drug 'Antrycide'. No one would wish to depreciate that brilliant achievement in chemotherapy; but can anyone who is familiar with the tsetse problem in Africa believe that it can have been solved by the use of this new tool alone? And yet, in the Northern Territories of West Africa (admittedly a special case—but *all* problems in applied entomology are special cases; that is why we need research) in the Gold Coast it has been proved by Morris (4) that sleeping sickness can be reduced by 98 per cent. simply by removing from the riverine habitat of *Glossina palpalis* and *G. tachinoides* a few species of evergreen trees and shrubs, and so changing the environment in the dry season retreats of these flies just enough to render them untenable. In South Africa the Pine Thrips *Heliothrips hæmorrhoidalis* has been controlled simply by thinning out the trees to give them more light and air.

The virus diseases of potatoes in this country are carried mainly by Aphids, particularly by *Myzus persicae*. Seed potatoes largely free from virus can be raised if conditions are unfavourable for the migration of winged Aphids to the potato crop. These Aphids overwinter in numbers in localities where crucifers are grown on a large scale. High atmospheric humidity and exposure to the prevailing westerly winds inhibit migration. Bearing these factors in mind it has been possible to select good sites for growing virus-free seed potatoes in North Wales, South Devon, and other places. That is a crude example of applied ecology. But there can be little doubt that with more understanding of the behaviour of Aphids we shall be able to manipulate the cultivation of many of our crops in such a way that we can come to terms with the Aphids that affect them.

One would find no difficulty in adding endlessly to these examples. All I wish to do, without discouraging for one moment the study of insecticides and the search for new toxic materials, is to emphasise the increasing scope and need for entomological research—to guide intelligently, to supplement, and sometimes even to supplant the application of chemical methods. Far from being exhausted, I believe there are still unbounded opportunities within the science and practice of entomology.

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THE UPLAND PLAINS OF BRITAIN: THEIR ORIGIN AND GEOGRAPHICAL SIGNIFICANCE

Address by

PROF. S. W. WOOLDRIDGE

PRESIDENT OF SECTION E

1. My aim in this address is to survey some of the problems of the later stages of landscape evolution in Britain and more particularly the evidence afforded by the various well-marked erosion surfaces developed on rocks of widely varying ages. My subject, therefore, falls within the strict and proper purview of Geomorphology, and during the last 30 years work upon it by both geographers and geologists has been very actively pursued in Britain. The time seems ripe for a review of such work; if it cannot provide answers to all the problems arising, it may at least succeed in asking some of the right questions and thus guide the direction of further research.

If in pursuing this subject we find ourselves cultivating the common borderland of geology and geography I find no need to apologise for that here. It is clearly part of the tradition of the British Association that inter-sectional interests and issues should be discussed. For myself, I do not doubt the close relevance of the physiographic findings to the general tasks of geographical interpretation. That relevance has been evident to a large number of geographers and to it we certainly owe the initiation of the 'Terrace Commission' of the International Geographical Union which has been at work since 1926. The instinct which prompted and still maintains the investigation is sound enough. One can draw no tidy division, armed with 'keep out' notices, between the fields and interests of geologists and physical geographers. I have no desire to enter upon one of those methodological disputations of which we, as geographers, are perhaps inordinately fond, and I will content myself with affirming that, if the study of the shape and shaping of the physical landscape is ever expelled, through prejudice or ignorance, from the ambit of geography, our subject will have lost much of its historical birthright, including even the right to its time-honoured name. Welcoming cordially as I do the marginal growth of our subject into the fields of study of 'social man,' I flatly rebut the suggestion that this alone is 'real geography.' Like geomorphology itself, it is part and only a part of a whole. As I see it, one of the emphases at present needed in British geography is a reminder of the virtue of keeping both feet firmly on the ground—'the solid ground of Nature,' if not quite in the sense in which Wordsworth first used this famous phrase. For 'ground' you may read 'land' or, if you insist, 'landscape.' But let us at least be clear that landscape involves more than the so-called cultural landscape. However important a role we ascribe to the latter, we shall make but little of its study without much closer scrutiny of its physical foundations than is now fashionable in some quarters.

2. By a combination of physical chance and human perversity the Tertiary era has almost become a neglected 'Dark Age' in the geological history of Britain. Despite, or perhaps because of, the general wealth of our geological record in these Islands there is as yet no adequate interpretation or even description of their geomorphology. The geologist deals convincingly and at length with what has been called palaeogeography, but the

geographer turning to these findings for the antecedents of the features and phenomena of his study is too often handed a stone in place of the relevant bread. For palaeogeography is necessarily and inevitably an affair, primarily, of long-past sea-bottoms and of coastlines tentatively located. The evolution of the Tertiary land surfaces, not less a proper enquiry in palaeogeography, presents quite different problems, amenable to attack only by the methods of geomorphology.

These problems are rendered difficult of solution by the limited and fragmentary character of our Tertiary sedimentary record. This default of evidence, taken with the fact that many of the British uplands show manifest structural lineaments of ancient date, and present seemingly simple relations to the lowlands, puts a premium on over-simple explanations of the physical features of Britain. They are coloured, too, by the widely received conclusion that the Upper Cretaceous transgression left little of the British area unsubmerged. The widely-spreading sheet of Chalk, or equivalent deposits, uplifted in early Tertiary times, becomes as it were the *terminus a quo* of British landscape evolution, and, to a first approximation, all that seems necessary is to conceive of the stripping of this Cretaceous mantle from upland and lowland alike, thus allowing the older structural patterns to show through. More than a century of detailed work on the succession and structure of Britain enables us to give graphic and circumstantial accounts of the antecedents, climax, and sequel of the great episodes of Caledonian and Hercynian mountain-making. The faulting which accompanied or succeeded the latter is still plainly legible in the marquetry of the British landscape and we can trace, in gratifying detail, the later story of the Triassic deserts and the Jurassic and Lower Cretaceous seas, until the great Upper Cretaceous transgression closes and, as it were, seals off the account. It is, naturally enough, such episodes as these which have always taken pride of place in the tale of British palaeography as written by pioneers like Edward Hull (1) and A. J. Jukes-Browne (2) or in broader outline by geographers seeking an evolutionary background to their canvas. The later parts of the record, excepting only the Alpine folding in the South Country, receive much scantier treatment in the standard accounts, though they are obviously much more important for the student of present surface forms. Jukes-Browne, indeed, made a brave and ingenious attempt to trace the progress of Tertiary denudation; but he was necessarily denied much of the evidence now available and many of his conclusions are no longer tenable. Later Professor L. J. Wills (3) very clearly and fairly summarised the more recent work done in South-East England, but he did not find occasion to deal with the landscapes of Devon and Cornwall which probably hold not a few of the keys to the interpretation of our Tertiary land surfaces.

In the light of the evidence gained during the last 30 years it is now time that geologists and physical geographers faced the fact that one cannot, in any real sense, explain, say, the Welsh mountain mass in terms of Caledonian orogeny, which was a phase not so much in its history but its pre-history. Still less are the land forms of Devon and Cornwall at all closely related to the Hercynian folding; they bear the plainest impress of Tertiary faulting and late Tertiary planation. To elide, as we constantly tend to do, some 65 million years of geological process, accountable to Tertiary time, is to import a sort of 'antiquarian' squint into our physiographic interpretations. Since Tertiary deposits obtrude so little, and over much of Britain even Cretaceous rocks are missing between the solid basement and the Pleistocene drifts, physiographic accounts perform a sort of leap-frog antic and pass in one breath from tracing the topographic expression of ancient structures to the story of glaciation. Since no deposits intervene, it has been widely assumed that nothing can safely be said of the vast 'lost interval,' or that it is a field, at best, for aimless and profitless speculation. It is this great lost interval in the geological story, and a corresponding area of ignorance in the interpretation of the geographer's landscapes, that the methods of modern geomorphology are fitted to fill. To ignore them and to leave the lost interval intact is as if we sought the roots of some modern politico-social problem

in minute and careful studies of the manorial system, ignoring the immediately precedent stages of history. Or it may afford a closer parallel to our case if we imagine an archaeologist reconstructing the original form and stages of decay of some major building of the past. Let it be assumed that it was fashioned of local building stone. Even so the grain or other internal structures brought out by weathering in the building blocks would not form valid parts of the archaeologist's reconstruction. The plan and fashion of the original building and the successive phases of its demise would reflect quite other processes than those which formed or weathered the stone. We must not, of course, press this analogy too far, for, in our case, the great principle of geological continuity is at stake. The later are of one piece with the earlier episodes as parts of a continuous physical evolution. But if as Professor O. T. Jones recently noted (4) the greater part of Britain has been structurally dead since the close of the Hercynian movements it has none the less been morphologically very much alive. If the structural geologist has little or nothing to say of the vast periods which appear to him quiescent, the geographer must needs turn to geomorphology for guidance in his own analyses.

3. In broad terms there are two methods of tackling the problem of the lost interval. The first consists in the analysis of the drainage system and its evolution. It was first applied in this country by W. M. Davis, followed by Cowper Reed, Buckman, Strahan and Lake and, in more recent years, notably by Linton and Bremner. I shall not attempt to deal here with the method and its findings. Arguments of considerable elegance can be based upon it, but they tend, of themselves, to be indecisive unless definite stages in chronological sequence can be established. This the second method supplies, for it aims at the recognition of past base-levels of erosion whether represented by submarine or sub-aerial surfaces. Associated with it is the analysis of composite or cyclic river profiles first applied in this country by O. T. Jones in 1924 and since developed by the late J. F. N. Green. I shall not discuss it at length since it is definitely ancillary to the study of erosion surfaces. Miller has shown the wide margins of error which characterise the method of base-levels determined by simple extrapolation of river curves. Without the check imposed by erosion surfaces and river terraces, reconstruction of past river profiles could hardly yield dependable or definitive results. Far greater importance must be assigned to plateau-like erosion surfaces, for these are actual geographical features which can be mapped with a reasonable approach to accuracy and traced for considerable distances. Only the preliminary question of nomenclature need detain us before we review on a regional basis the considerable body of evidence now to hand.

4. It is usual to refer to the features in question as ' erosion surfaces ' or ' erosion platforms ' and in ordinary working parlance these terms are unobjectionable and too well entrenched to be easily replaced. Yet neither term is free from ambiguity, and since geographers are showing timely signs of a certain tenderness of conscience in the matter of terminology it will not be amiss to employ a little circumspection. It is clear that there are many erosion surfaces, e.g. valley sides and cliff faces, which are in no sense platforms. The term ' platform ' is graphic and self-explanatory, and in some cases apt enough, yet the purist may insist that a platform should be flat or nearly so. In fact, most of the surfaces in question are appreciably inclined, whether by virtue of original slope, subsequent dissection or tectonic warping. Moreover some authors use the term ' structural platform ' where the top of a resistant master-stratum is locally stripped of a conformable overburden. Such a platform is evidently, in some sense, an erosion surface, though not generally in the sense in which ' the platform school ' use the term. It is, of course, obvious enough that the significant features for our purpose are surfaces of low relief inferred from accordant summit levels or preserved as flats truncating or bevelling the structure. It is to be noted that we cannot use a genetic nomenclature without begging the very question we are required to solve. Some of the surfaces are no doubt peneplains in the Davisian sense, but unless that term is given a grossly widened and weakened connotation

it cannot be applied to them all. I do not favour the use of the term marine peneplain. Nor is this all. Extensive surfaces of low relief bevelling structure may be neither peneplains nor strand-flats, but the product of lateral river corrasion, arid planation or advanced corrie sculpture. There is the further difficulty that a surface may be 'fossil'—i.e. an exhumed plane of unconformity, which commonly leaves us in a further doubt whether its original fashioning was submarine or subaerial, or in part both. To press these difficulties to the limit would no doubt be pedantic, yet they are real enough if we seek a definitive and unambiguous glossary definition of erosion surfaces and platforms. I shall not forgo the use of these simple and essentially commonsense terms in reviewing the British evidence, but my title revives a practice of the Geological Survey in Devon and Cornwall and notably of Mr. Henry Dewey. Generically, as geographical features, the chief members of the series of surfaces are 'upland-plains.' In using this term one begs no question at all and merely states an observed fact. Even those who are sceptical of the reality of erosion surfaces must surely grant that upland plains are a conspicuous feature of the physiography of Britain. There is the further advantage that the term is applicable to composite surfaces, peneplains retaining an appreciable relief, or a series of closely-spaced platforms difficult or impossible to separate. I will not pursue further what might easily become an irritating discussion. I submit, merely, that to speak of the 'upland plains' of Britain conveys at once more and less than the 'platforms' of Britain. If anyone thinks that I might as well have said the plateaux of Britain, I will ask leave to express my personal disagreement without further argument.

5. I must attempt now the far from easy task of summarising the evidence at present available under regional headings.

If I begin in South-East England it is not because this is the region I personally know best, but because here Tertiary deposits, and folding of known age, put our problem in its correct time setting. The cardinal feature is the Pliocene platform developed on the Chalk outcrops flanking the London Basin and either bevelling the escarpment or forming a distinct bench on the dip-slopes (5). The deposits which rest on it, originally ascribed to the Older Pliocene have since been shown to yield a Newer Pliocene (Red Crag) fauna (6). The typical elevation of the platform and one faithfully maintained is about 600 ft. but it ranges downwards to 550 ft. or perhaps slightly lower, while what has been taken as a degraded cliff-line feature at its back comes on somewhat below 700 ft. Where the platform adjoins higher ground, as in the North Downs of Surrey or Kent, or in the Chiltern Hills, this ground shows distinctive features both in its drainage patterns and soil cover which are consistent with the conclusion that it was not submerged at this time. On the platform the drainage lines could not arise till the Pliocene sea floor was uplifted, and they must have been ultimately superimposed from the cover of sand and shingle. With the help of this clue the coastline was traced by Professor Linton and myself southwestwards from near Hitchin to the neighbourhood of Dorchester (7). It occurs also in a dissected condition on the South Downs, but we concluded that much of the central Wealden area remained unsubmerged.

Within the Chalk areas the recognition of the Pliocene platform defines also not only the higher tracts but the exhumed sub-Eocene surface. The former areas have been regarded as surviving relics of the Miocene or Mid-Tertiary peneplain. The latter forms a distinctive facet on the lower part of the Chalk dip-slope. Only rarely as in the Western Chilterns does it form a true upland plain. Its existence reminds us that a lengthy period of erosion ran its course before the earliest Eocene deposits were laid down : there is evidence that the Chalk cover was locally breached by early Eocene times. Though the sub-Eocene surface was locally trimmed by the waves of the Eocene sea it was essentially a peneplain, and the possibility that it may prove recognisable in northern or western Britain must be borne in mind.

Between the Pliocene platform and the 100 ft. or Boyn Hill terrace of the Thames, a number of gravel-covered valley-floor plains can be distinguished in the area north and west of London, but at two levels only is there evidence of wide-

spread planation. What has sometimes been called the 400 ft. platform dominates the form of the country north of London (8). It forms the summit plain of Middlesex, South Hertfordshire and much of Essex, though in the latter area it nowhere attains 400 ft., dropping eastwards to 350 ft. or below. I formerly regarded this feature as a river plain and there can be no doubt that the ancestors of the Thames and its southern tributaries flowed across it and left deposits upon it; but I am now inclined to revert to the view of George Barrow that the greater part of the deposits (Pebble Gravel) are marine though later in age than those of the main Pliocene platform. Evidence of a still-stand at about the same level is found in the Northern and Central Weald though there is here no suggestion of marine planation.

At a lower level what I originally termed the 200 ft. platform is more widely represented (9). It shows every sign of being a surface of sub-aerial planation and was formed before the major glaciation of the area. As we shall see, a comparable feature has since been widely recognised in Britain : in part at least it corresponds to the Ambersham stage of J. F. N. Green. I must plead guilty to having tentatively correlated this feature with the Milazzian marine stage of the Mediterranean Pleistocene. I have often since been rather aghast at my carefree rashness in so doing, but the suggestion still agrees in broad terms with the local evidence and I note that Professor Zeuner accepts the correlation.

A recent contribution to the denudation chronology of South-East England is the work of Mr. B. W. Sparks on the seaward slope of the South Downs (10). Here he recognises no fewer than eight dissected shelves between the Pliocene platform and the well-known Pleistocene raised beach of Goodwood. They range in height from 475 ft. to 180 ft. with an average interval of about 42 ft. The best developed of the features are those at 475 ft. and 430 ft. respectively, while those at 345 ft. and 180 ft. are relatively feeble. None of the features attain the magnitude of an upland plain : they are narrow facets on the dip-slope. To those who find the number of stages embarrassingly large I will only say that the record deduced by Sparks is wholly consonant with that of the area as a whole; and if we accept the fact of marine planation at the 600 ft. (Pliocene) and 100 ft. (Goodwood) levels, it indicates a consistence and expectable sequence of coastal evolution between those dates.

6. Passing now westwards we find that Southern Hampshire, despite its impressive terrace sequence, has little to offer us at the higher levels, though the New Forest summits east of Fordingbridge may retain isolated fragments of a 400 ft. platform. In tracing what we regarded as the main Pliocene shoreline across Wessex to the Dorset coast Prof. Linton and I fixed upon the break of slope above the Hardy monument near Dorchester as the back of the 600 ft. platform and this still appears to be a reasonable inference. Though the intervening ground awaits detailed scrutiny, less than 20 miles westwards, across the Devon boundary, the sequence of high level platforms is strongly resumed. Here in the familiar hinterland of Sidmouth, Seaton and Lyme Regis, where the flat-topped mesa-like masses of Upper Greensand rise above the Triassic basement, one is readily led into the facile assumption that the summit plain is a structural platform—i.e. the stripped surface of the Upper Greensand. This is not so. The surfaces transgress the structure locally, quite apart from the fact that they are developed to the east on the Chalk and the west on Palaeozoic rocks. J. F. N. Green (11) distinguishes a remnant of a 1,000 ft. platform on the crest of the Cretaceous escarpment south of Taunton. Below it the broad plateau between 920 ft. and 750 ft. is identified with Barrow's Bodmin Moor platform in Cornwall, while nearer the coast platforms appear with backs, in descending order, at 690 ft., 595 ft., 530 ft. and 505 ft. High valley-side flats in the Axe valley lie at about 440 ft. and may be regarded as grading to a shoreline at a slightly lower level not now preserved in the area. Green regarded all the platforms from 505 ft. upwards as marine. They present indeed the unmistakable aspect of strand-flats and the record of shingle on the Bodmin Moor surface near Otterford affords further support in this case. Though I am sure the last word has not been said on this

ground, and it remains for us to trace Green's features eastwards through Dorset I entirely accept his broad conclusions and regard them as of the highest importance.

Let us then turn northwards for a moment to the Bristol district where upland plains cut in Carboniferous and younger rocks are plainly in evidence. As regards the former as seen, for example, at Clifton and Durdham Downs it was for long customary to assign the planation to the work of the Rhaetic or Liassic seas. This is a good example of the recurrent tendency to interpret our British platforms as fossil surfaces, thus gravely underestimating the duration, and what may be termed the planation-potential, of Tertiary times. The theory is highly implausible here since the Lias has itself been subject to well-marked folding on east-west axes, probably of Mid-Tertiary date. A. E. Trueman stated the case for late Tertiary planation (12) recognising erosion platforms at 200–300 ft., 400–450 ft. and 550–600 ft., while a higher surface at 750–800 ft. is prominent in the Mendips, on Dundry Hill and on the high summits of the Cotswolds north of Bath. Four years' war-time evacuation at the University of Bristol offered me some limited opportunities of following Trueman's lead in this area but I will confine myself here to two observations. The Durdham Downs platform, so prominent a feature of the Bristol scene, must in my view be regarded as including a representative or continuation of the 400 ft. surface, though no clear-cut upper limit against higher ground can be fixed in the immediate environs of Bristol. The general level of the higher parts of the platform is 350 ft. and though it ranges down to the lower levels the same is true, as we shall see, in many parts of both Cornwall and South Wales. That the surface is at least post-Liassic was clearly proved by a large bomb crater in Westbury Park on the eastern side of the Downs : this revealed Lias levelled flush with the Downs surface cut in Carboniferous Limestone.

The other feature which greatly impressed me was the widespread planation at between 550 and 600 ft. in the Oolitic country south of Bath. North of the Avon valley the great bastion of Lansdown rises to 780 ft. and viewing it northwards across the lower surface, I was inevitably reminded of the feature I had mapped in the London area behind the Pliocene shelf. The nearest area of Pliocene planation surveyed by Linton and myself lies some 12–15 miles to the south-east on Salisbury Plain : if the same sea covered both areas the only direct continuity between them seems likely to have been *via* the breach in the high Chalk country near Warminster. Flint is widely spread over the high ground south and east of Bath and its distribution merits most careful mapping.

7. I have considered the East Devon and Bristol areas at this point of the argument since they form a link between South-East England and the South-West peninsula where striking evidence of planation has long been known. As early as 1890 Clement Reid associated the famous coastal platform at 430 ft. with the St. Erth Beds, assigned by him to the Older Pliocene (13). Not until the Geological re-survey of the area reached the Bodmin mass were considerable relics of higher platforms encountered ; but in 1908 (14) George Barrow described the 1,000 ft. and 800 ft. (Bodmin Moor) platforms here, and both were later identified on the flanks of Dartmoor. Barrow regarded both as marine and they certainly bear all the aspect of strand-flats, but Clement Reid, speaking later of the Dartmoor area, was inclined to invoke a sub-aerial origin.

There has been no general review of the Cornwall and Devon evidence since the completion of the re-survey nearly 40 years ago, though some local studies have been made. What is evidently required is a wide extension of the careful and detailed type of work which my colleague, Mr. Balchin, is communicating at this meeting. Meanwhile we may note that the whole of the tentative chronology which has been generally accepted, ascribing the 400 ft. platform to the Older Pliocene and the higher surfaces to the Miocene rests on Clement Reid's verdict as to the age and depth of accumulation of the St. Erth Beds. I suspect that this chronology is badly in error. The St. Erth beds lie at about 100 ft. O.D. and were accumulated, according to Reid, in some 40 fathoms of water. This would give a sea level at about 340 ft. and the

link of the low-lying fossiliferous sediments with the much higher strand-flat was thus accomplished. In the matter of age no great difficulty existed while the 600 ft. platform of South-East England was regarded as of Older Pliocene age. But since we now know that the deposits near London yield a newer Pliocene fauna, the discrepancy of level is serious and would imply warping, of which neither region nor the intervening areas shows any clear sign. Morphological evidence would in fact suggest a much later age for the 400 ft. platform. I believe it may well prove to be not older than latest Pliocene in age. This would bring it into line with the corresponding feature in the London District but would imply either the divorce of the platform from the St. Erth beds or a revised view of the age of their fauna. It would also imply that a 600 ft. surface should be recognisable in the South-West. In fact both Balchin (15) and Gullick (16) have found local indications of such a surface in Cornwall, and Balchin has also located it on Exmoor with its back at the usual level of between 650 and 700 ft. It is imperative that this surface should be carefully traced. A glance at the map reveals many areas which might include it, but there is also a strong suggestion of general sub-aerial grading to or towards the 400 ft. coastline, in the course of which initial surfaces between 500 and 1,000 ft. have been modified or destroyed. Such a grading process, of which Miller has found evidence in South Wales and elsewhere, was of course inevitable. It is clearly shown over many areas of the 400 ft. platform itself, notably on the south of the peninsula between St. Austell and Dartmouth. The sub-aerial imprint on older platforms is necessarily even more strongly marked, but I believe that it will be possible to distinguish residual areas of marine planation.

The existence of a lower platform at about 200 ft. in the area has also been claimed by several workers and there is, indeed, clear evidence of a still-stand at this level, though the surface often merges imperceptibly upwards into the 400 ft. strand-flat.

8. It thus seems possible to establish a base or reference line more than 300 miles long from Kent to Cornwall along which the platform sequence is everywhere broadly consistent, at least at the lower levels. It is a highly remarkable and significant fact that we can now run another line from Cornwall to Cumberland along which entirely similar evidence is found.

I can deal more briefly with this area since it is well covered by Miller's review of 1939 (17) and the important statistical analysis, based on altimetric frequency curves, published by Hollingworth in the same year (18). In South Wales the work of Miss Goskar, Dr. Trueman and Prof. George revealed a succession of platforms, up to at least 600 ft., closely comparable with that of Cornwall. Prof. Miller's work on the area emphasises the probability of a considerable element of later sub-aerial grading or merging of the surfaces. Miller then pressed the enquiry northwards along the coasts of Ireland and Wales to Cheshire, Lancashire and the Lake District, where he incorporated the results of the work by Hollingworth. He found recurrent evidence of marine still-stands at about 800, 450 and 200 ft. From the 200 ft. platform, which is widespread, a sloping surface or a succession of plateaux leads up to a presumed cliff-line at about 450 ft. Above this a similar surface, locally showing discontinuous cliffing, but often smoothly sloped, ascends to 800 ft., the flatter areas being generally best developed above 600 ft. Miller has also detected both coastline features in Southern Ireland. Hollingworth's analysis of the seaward slopes of the Lake District revealed six main levels of planation below 1,100 ft. (320, 430, 570, 730, 820 and 1,070 ft.) and three subsidiary levels at 520, 620 and 920 ft. respectively (19). A more recent study by E. H. Brown (20) of the hinterland of Aberystwyth claims evidence over the same range of height for major planation stages at 1,000–1,100 ft. and 500–675 ft.—the main coastal platform. Four minor intervening stages occur with upper levels at about 1,000, 875, 825 and 750 ft. respectively, while extrapolation of river profiles leads him to infer a series of lower sea levels at about 407, 326 and 233 ft.

9. I shall not attempt to pursue this review in detail into Scotland where less evidence is at present to hand, and my ignorance of much of the ground prevents

my commenting usefully upon it. We may note, however, that quartzite-flint gravels at an elevation of 350–400 ft. were described as long ago as 1921 in the Buchan district of Aberdeenshire and a Pliocene age has been tentatively assigned to them (21). In the Central Lowlands Ogilvie has distinguished his Higher and Lower Lowland peneplains (22). The lower feature is widely drift-covered and is an undulating surface which hardly suggests marine planation; but the Higher Peneplain, typically developed between 500 and 750 ft. might well prove to include one or more strand-flat features.

10. Our review has omitted the eastern coastal areas south of the Border, since the evidence is at present slight and inevitably suspect. The attitude of the Pliocene base in East Anglia and the Low Countries, and the great thickness of Pleistocene deposits in the latter area, show that down-warping of the North Sea depression must have been active during and after Pliocene time. It is clear that the basin is in essence geo-synclinal in its nature, and its growth began in Lower Cretaceous times if not earlier. It is a reasonable presumption that our eastern coastal areas have been affected by this warping. In the south this is beyond question; and I believe one can locate the effective margin of the North Sea depression in mid-Essex near Chelmsford. For many miles northwards we have no likely source of evidence on this point, though it is obviously desirable that the morphology of the Lincolnshire and Yorkshire Wolds should be closely examined. Versey has examined possible relics of Tertiary deposits on the Yorkshire Chalk (23). It is significant that his application of Hollingworth's method of altimetric analysis to the Cleveland area fails, on the whole, to show clear evidence of planation at the usual levels. It seems possible that there may be some planation at and below 800 ft. in the coastal zone between Saltburn and Scarborough, but in default of a demonstrable sequence of platforms no correlation is at present possible. Hollingworth included the Cheviot area in his statistical survey, finding evidence of planation at 560, 760, 920 and 1,070 ft. but not at 400 ft.

11. We have so far ignored the higher levels, though in the broad connotation of the term they include some of the most extensive upland plains in Britain, including the summit plains of the Welsh Mountains and the Grampians. If it be tentatively assumed that the platforms at and below 1,000 ft. are of late-Tertiary marine origin, it is at these higher levels that we might hope to find representatives of one or both of the two sub-aerial peneplains of South-East England, the sub-Eocene and the Miocene surfaces, though it is far from certain that they would be separately recognisable. Both of them, and especially the first, might be expected to record appreciable warping. I think there can be little doubt that the summit plain of Wales includes representatives of one or both of these surfaces. Similarly, I note with satisfaction J. E. Richey's conclusion that, since the higher peaks of the Tertiary volcanic area of Western Scotland attain to 2,000 or even 3,000 ft., the igneous rocks were involved in the great summit plateau of the Highlands (24). It would evidently be most remarkable, if not quite unaccountable, if the great periods of Tertiary sub-aerial base-levelling, and notably that which followed the Alpine folding in the south, were not represented in our upper mountain levels. There are, I believe, two extreme misinterpretations of the evidence of which we have to beware. On the one hand I must reject both of the alternative suggestions that these high surfaces are exhumed sub-Cretaceous or sub-Triassic planes. Therein lies the old-established heresy of virtually ignoring Tertiary time. On the other hand I view with some disquiet what I may term the nibbling at the high summit surfaces from below by the multiplication of supposed platforms on map evidence alone. We must follow where the facts lead; but let us be sure that they are facts and not overlook the wide spacing of our surveyed contours at the higher levels. We may recall that Hollingworth's altimetric analysis, taken as a whole, shows no major frequency maximum between 1,000–1,070 and 2,000 ft. On the other hand we must note that H. Fleet (25) found surfaces in the Highlands at 2,300–3,000 ft., 1,400–2,000 ft. and 750–1,000 ft. Hollingworth finds some evidence of planation at 1,170 1,340 and 1,600 ft. in the Lake District

and McConnell (26) distinguishes surfaces at 1,700–1,850, 1,500–1,600 and 1,250–1,400 ft. in the Howgill Fells. Similarly, Miss Goskar (27) subdivided the Welsh high plateau at 1,450–1,500 and 1,200–1,300 ft. while more recently E. H. Brown finds some evidence of planation at 1,200–1,300 and 1,450–1,500 ft. in the Plynlimon area. Among the results of this kind which I regard as most trustworthy, having watched the progress of the work in my own department, are those obtained by Balchin on Exmoor and based upon accurate field work. He claims recognisable surfaces at 1,000–1,225 ft. and from 1,250 to 1,600 ft., the last being what I should call an ' envelope surface ' tangential to the culminating points.

In his last most valuable and suggestive paper on the Dartmoor area J. F. N. Green (28) found the highest evidence of an unwarped erosion surface, conceivably marine, at about 1,150 ft. He suggests that the higher summits of Dartmoor between 1,564 ft. (Rippon Tor) and 2,039 ft. (High Willhayes) closely fit a plane which slopes 30° south of east at 1 in 130. He further suggests that this is compounded of an eastern slope of 1 in 160 and a later superimposed tilt of 1 in 260. This I conceive to be the type of evidence which ought to be yielded by our higher upland summits. It would be a highly suspicious circumstance if we could find no evidence of warping at the higher levels. The multiplication, on fragile and ramshackle evidence, of supposed flats up to the level of the highest summits would confront us with features extremely difficult to explain, and if their reality were demonstrated they would throw doubt on the interpretation of the better marked record at lower levels. In default of cogent evidence to the contrary I therefore accept the conclusion of Trotter (29) that the surface of the High Pennines in the Alston block is essentially a sub-aerial peneplain and, with rather more doubt, Versey's similar conclusion concerning the summit surface of the North York Moors.

12. Even if we leave aside the more debatable evidence from the higher levels the facts reviewed offer a highly significant body of data which challenges interpretation in the wide over-lapping sector shared by geophysics, stratigraphical geology and geomorphology. On the general and geophysical aspect of the question at issue I do not consider that it is yet possible to add usefully to the shrewd and balanced discussion published by Prof. Baulig in 1935 (30). If we seek support for the general hypothesis of late-Tertiary eustatic shifts of sea level, it can hardly be denied that Britain affords it in very notable degree. Baulig emphasised the wide recurrence of his three dominant levels, at about 180, 280 and 380 metres throughout France and the Mediterranean area. Of these at least the two lower members have close parallels in Britain, and the third is present, if less widely known. But the British record is in some respects both more detailed and, as I believe, more accurately based than that of France, so that it might fairly be claimed that it yields independent evidence for the eustatic hypothesis. J. F. N. Green (28) recently quoted J. Ball's estimates of the successive Pliocene and early Pleistocene sea levels of the Mediterranean (Middle Pliocene 591 ft., Late Pliocene 505 and 423 ft., Early Pleistocene 338, 236 and 187 ft.) and noted how closely these correspond with his own findings in Britain.

But I must forego here any temptation to pursue the primrose path into the seductive quagmire of long-distance correlation. There are, for the moment, sufficient British problems of correlation confronting us and we have by no means yet done sufficient field work to settle them. The cumulative force of the evidence in favour of eustatic movements is indeed very strong, but it cannot yet quite sustain a rigorous proof of the absence of warping, nor, on the other hand, does it suffice to locate or evaluate any such warping. It is clear enough that among the workers of the ' platform school ' there are found representatives of both those widely-spread types of mind which we may term the ' lumpers ' and the ' splitters.' The two points of view need not be unalterably opposed, but it remains true that whether or not one finds apparent evidence of warping depends, often enough, on the number of distinct platforms which one is prepared to recognise. We do well to remember therefore, that if, as seems probable, we have evidence of one or more

advances and regressions of the sea, the sea has stood at least once, and perhaps more than once, at every level within the total range of height affected, and a great multiplication of platforms is to be expected. Nevertheless, as Miller has emphasised, the final stages of retreat must necessarily have left the higher members of the series exposed to sub-aerial attack and a certain breaking down or semi-grading of the marine stairway must inevitably have followed. The resulting surface however, cannot, in my view, be properly termed a peneplain save in a purely descriptive sense. It is in no sense comparable with that long-term product of sub-aerial wastage which we conceive as terminating a true cycle of erosion, but is simply a composite surface of marine planation modified by sub-aerial exposure.

A further problem of interpretation is raised by the question :—Were the strand-flats cut during the advance or retreat of the sea, or during both? Here we must not ignore the general evidence of stratigraphical geology. I have always rejected in my own mind the facile conclusion that as we climb the platform stairway we move steadily backwards through Tertiary times, so that if the lower platforms, up to say 700 ft. are taken as Pliocene in age, those immediately above them are, *ipso facto*, Miocene. No facts at present before us really warrant this conclusion though it remains a possible hypothesis. But if we choose to entertain it we must recall that Europe as a whole gives evidence of distinct Miocene and Pliocene transgressions separated by a phase of regression. At its greatest extent, in Middle Miocene times, the Miocene sea was certainly not far from the British area, but I have no doubt that geologists will remain sceptical of its actual presence within Britain, unless palaeontological evidence comes to hand. In any case it seems an inescapable conclusion that, if any of our higher 'strand-flat' features were formed in Miocene times, both they and much ground at lower levels must have been bared to sub-aerial attack before any Pliocene transgression. For my own part I am content for the present to remain warily agnostic as to any recognisable Miocene phases and to conclude that, at least over much of Western Britain, a Pliocene transgression, probably equivalent to the Calabrian stage of the Mediterranean, left evidence of planation up to a height of 1,000 or even 1,200 ft., and that the successive platforms at lower levels mark stages of retreat. I am not satisfied that we have yet devised sound criteria which might enable us to distinguish an 'ascending' from a 'descending' sequence of platforms, but the mutual relation of our platforms below 1,000 ft. certainly suggests a descending sequence.

If this conclusion be accepted there is some suggestion of a differing upper limit of marine planation in the Western and South-Eastern areas respectively. I can see no evidence at all of marine planation in South-East England above 700 ft., but many indications that the higher areas have been sub-aerially fashioned throughout. There is, no doubt, ground for difference of opinion here, for at best the evidence may be deemed indecisive. But while any doubt remains we must keep an open mind on the possible presence of warped surfaces. The general tenor of the British evidence as a whole admittedly points clearly to stability over wide areas with either uniform uplift or uniform regression of the sea. The local tilting or faulting so often invoked in older interpretations of British physiography is, in general, quite unacceptable. If I may borrow from the parlance of the essentially geomorphological game of golf, it has often constituted a sort of tectonic 'niblick' employed in emergency to get the investigator out of 'bad lies.' But that is not to say that over wider areas we have yet attained a final demonstration of absence of warping, though I still hope and believe we may do so. It will require more than analysis of existing maps. Careful and objective field work alone can fill up the gaps in our evidence.

13. Whatever doubt remains concerning the detailed chronological interpretation of the evidence, there can be no doubt at all concerning the nature of the general physiographic picture revealed by our upland plains. They throw almost the only available light on the earlier stages of coastal development and much also upon drainage evolution.

It has long been an anomalous gap in

British physiographic history that the stages linking the Tertiary coastlines with those of Pleistocene times and the present day are almost completely unknown. Jukes-Browne, (2) noting the evidence of the northward retreat of the Pliocene sea in East Anglia assumed a general regression in Upper Pliocene times to the vicinity of the 100 fathoms contour line. This conclusion has been widely and uncritically accepted, but it is wholly erroneous. Apart altogether from the evidence of marine platforms, it is inconsistent with the known history of Pleistocene times. It would imply—and this conclusion has in fact been drawn—that all our coastlines and their Pleistocene predecessors were fundamentally ' coastlines of submergence ' a general positive movement of the strand-line having over-ridden the fluctuations due to the varying volume of the ice-sheets. Of such movement there is neither any evidence nor even any plausible explanation. In fact, as we have seen, the Upper Pliocene coastline, successor to earlier and higher strands, was probably roughly parallel to the present coast and nowhere far from it. This conclusion is wholly consonant with the distribution of the earlier Pleistocene raised-beaches. From the maximum stage of the Pliocene transgression the general nature of British coastal evolution is clearly indicated by such work as that of Sparks in Sussex and Balchin on Exmoor. The sea withdrew uncovering in sequence a series of ' Ararats.' In Cornwall the ' Lands End ' of the 400 ft. stage was on the western side of the St. Austell granite, leaving the Carnmenellis and Land's End granites as offshore islands ; at an earlier stage of higher sea level they may have been archipelagos closely comparable with the present Scilly Islands.

14. The related problems of drainage evolution are distinctly more complex. In some areas, e.g. the flanks of the London Basin at the 600 ft. stage, the drainage direction on the contemporary land was normal to the strand-line and emergence brought simple extension across the strand-flat of streams draining to the former coast. Even in this area there may be some doubt as to how far marine planation and the spreading of a thin sedimentary veneer completely obliterated the pre-existing topography. In some cases at least the extended rivers must have followed the line of their former courses fairly closely. But taking the south-eastern area as a whole, in the light of Linton's work in Wessex (31), there is little doubt that we have evidence of an episode of ' wholesale ' superimposition.

We have only to turn to Cornwall and Devon to perceive much more complicated and enigmatical drainage relations. Here is no simple case of coastward drainage ' extended ' stage by stage over a descending series of strand-flats. In profile the rivers admittedly give the clearest evidence of an intermittent negative change of base-level. As shown by Dewey (32) the innumerable minor coastal gorges, well illustrated near Tintagel, mark the response to emergence of the ' 400 ft. platform.' The Lydford gorge is similarly incised in the Bodmin Moor platform, while on the eastern side of Dartmoor the Becka Brook descends in a waterfall from the Bodmin Moor platform and traverses a deep gorge to join the River Bovey. In plan the relation of the rivers to the platforms is far less evident. The southward flowing trunk-streams (Exe, Tamar) rise close to the northern coast and appear to reflect a southerly tilt of the peninsula not shown by any of the platforms. This tilting may well date back to Oligocene or early Miocene times. If so we can only conclude that, as the later emergence proceeded, the rivers resumed their old southward flowing lines undeterred by the consequences of marine planation. If this was the sequence of events it affords independent physiographic evidence that the platforms are relatively narrow strand-flats and never extended far beyond their present outer edges. In other words, it would seem that the transverse depressions of the peninsula, such as that followed by the Tamar valley, must have been early established by earth movement, possibly faulting, and further developed by sub-aerial erosion before the Pliocene transgression. The Bovey Tracey-Petrockstow depression, which crosses the peninsula from side to side and may be of the nature of a rift valley, may well afford us a clue as to the nature and age of such features. It is pre-Upper Oligocene in date and, as Clement Reid surmised (33), the associated

faulting may well be contemporary with the Alpine folding of South-East England.

Even upon this hypothesis the rivers of the South-West still present many anomalies; even head-water streams at comparatively high levels follow lines more nearly parallel than normal to former coastline features. Moreover there are many cases of diverted courses, simulating the effects of normal river-capture, but evidently related to the successive stages of the retreating strand-lines. An instance in which such diversion is imminent is seen near Bude. Around Bridge, some five miles west of Holsworthy, the Tamar is incised in a plateau with summits at 450 ft. and with its valley floor at 300–350 ft. A mile or less to the west lies the degraded 400 ft. cliff of the Bude embayment and it is in course of sculpture by minor streams descending steeply to the coast. The westward diversion of the head of the Tamar at a point near Whitstone and Bridgerule Station may well ensue at no distant date. The Fowey to-day turns westward by a marked elbow north of Liskeard. A former southward course is marked by a wind-gap at 643 ft. at Redgate. It turns sharply south again north of Lostwithiel, but a former continuation of the westward course below 500 ft. is readily traceable. Barrow considered that this continued westwards to Newquay Bay (32); if so it was early diverted southwards by the Luxulyan stream and subsequently by its parallel neighbour draining south past Lostwithiel. A third diversion which caused a major modification of the morphology of the peninsula, was the reversal of the Taw, formerly tributary to the Exe, near Crediton, though the relation of this episode to the stages of retreat of the strand-line is yet to be worked out.

15. The light thus thrown on coastal evolution and river development suffices to show how vital to the general explanatory description of British land-forms is the evidence afforded by the platform sequence. Unless or until geographers adopt the attitude of Gallio and decree that they care ' for none of these things ' the general geographical justification of my theme is evident enough. But the sequence of land-form evolution I have reviewed has other and even more direct implications for geographical analysis, in its bearing on soil formation and land classification. If our successive upland plains represent surfaces exposed for different periods to atmospheric action, this must plainly be a factor in pedogenesis. I recently ventured to call the attention of pedologists to this factor (34) and by way of illustration cited the observations of Saint in the Barbadoes (35). In an area where the parent material is coral limestone the soils grade from red above 700 ft. through intermediate stages, to black below 200 ft. and the facts were interpreted as marking stages of uplift of the island and the varying exposure to weathering of the successive emergent zones. Whether or not the physiographic hypothesis is, in this case, sound, the idea of a ' maturity sequence ' is plainly implicit and must obviously be valid elsewhere. Recent years have seen some divergence in approach and emphasis between soil chemists and geologists in the study of soils. There has been a tendency to minimise the importance of the geological control of soil character and to call attention, quite rightly, to the numerous cases in which soil character is far from a simple and direct reflex of ' parent material.' There are, of course, many well-known reasons for this which I need not mention here, but among them is the question of the ' age of the site ' in a much fuller and more fundamental sense than pedologists generally imply in using this phrase. The pedogenic processes, both geo-chemical and bio-chemical, are, within limits, cumulative in effect and functions of time. Is it not an inescapable conclusion that two surfaces, developed on the same parent material, of which one has been exposed twice, three times or even ten times as long as the other, will show significantly differing soil profiles? We cannot claim *a priori* that this factor must be dominant, but merely that it exists and must be allowed for. It is not unlikely that, in general, bio-chemical is attained more rapidly than geo-chemical equilibrium in the soil profile, and may mask or even completely overpower the effects of differing duration of weathering. It is clear, moreover, that many of our upland surfaces have been substantially modified not only by glacial erosion and deposition

but also by long-continued human action. But it is no answer to the point I am making to surmise, without further enquiry, that the effects would be trivial or vestigial. We should expect them to be most clearly in evidence as between sub-aerial, marine, and 'fossil' surfaces where such are juxtaposed. South-East England (7) presents us with just this case; the crest region of the Chalk, above the main Pliocene coastline, the Pliocene shelf itself and the stripped sub-Eocene surface of the lower dip-slopes are found in close association. So far as 'solid geology' is concerned all these three areas are developed in Chalk, if we ignore the quite scanty relics of the former Pliocene cover. The geographer who is content to take his physical basis in crude and over-simple terms cannot but be aware that these so-called 'Chalk areas' differ widely, both in semi-natural plant cover and land utilisation. He is tempted to explain the differences solely in terms of the vagaries of human activity and to don a self-righteous halo for avoiding the heresy of determinism. But in fact it may readily be shown that the actual soil conditions differ widely on the three distinct surfaces. The sub-Pliocene and sub-Eocene surfaces are almost dramatically juxtaposed in East Kent with a hedgeless 'champion' landscape on the latter and a near approach to 'bocage' on the former. The contrast, however, is not merely in the soils of the residual flats, but in every detail of dissection and morphology. The contrast in the Miocene and sub-Pliocene surfaces is apparent both in Surrey and West Kent and also in the Chiltern Hills. The true Clay with Flints is largely, if not entirely, confined to the former surface.

Time will not avail me to multiply examples of such contrasts. I have observed them, in terms of soil profile, over the great expanses of the Weald Clay in Surrey and Sussex where the surface of the 200 ft. platform reveals plain evidences of stronger leaching than lesser and lower surfaces of more recent date. In this and other cases I believe that a detailed survey of soil-acidity would yield a pattern closely correlative with that of morphology. Nor can I doubt that maturity sequence expressed in soil will prove a valid factor in differentiating our upland moors. The several plant communities and their variants respond, of course, to the direct effects of form—of flat and slope and their immediate consequences in hydrology and micro-climate. But these same differences have been at work for even longer periods on soil and sub-soil and have left a legacy which we must disentangle from that of other factors.

16. Here then, if such be needed, is the direct 'economic' warranty for the concern of the geographer with the details of denudation-chronology, and I may finish where I began by stressing the proper geographical relevance of the subject I have sought to treat. The story of our upland-plains continued in that of valley-floor flats and terraces, is admittedly an 'evolutionary study' and indisputably a contribution to geological history. But without it there can be no 'explanatory description' of many British land-forms, nor will the geographer know sufficient of the nature and origin of the very land-surface itself to treat effectively of its successive human transformations. It may be an overstatement to say that all geographical knowledge, physical or social is, in the final analysis, historical knowledge, but it expresses a vital truth. I reject, in any case, the view that it is *only* as a means to a rational method of description of the earth's surface that geomorphology serves the geographer. We certainly are committed to study it as 'the field of organic activity and the abode of man' but we cannot do so without the fullest acquaintance with the manner of its fashioning. While, therefore, the 'platform story' has as much to contribute to geology as to geography I do not find it unaccountable that geographers have been actively interested in the problem I have surveyed. In any case I hope you will find it possible to bear with the attempt I have made as your President to maintain the link of our subject with geology and the related natural sciences to which it has always owed so much.

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ECONOMIC PROGRESS—RETROSPECT AND PROSPECT

Address by

PROF. G. C. ALLEN

PRESIDENT OF SECTION F

THE economists of today are no less ready than their predecessors to proffer advice on the ordering of affairs. Indeed, if there are any among our administrators who think that this advice is important, they must be embarrassed at its profusion and they may well feel every sympathy with the plight of the Confucian disciple, of whom it was said: 'When Tzu-lu heard any precept and was still trying unsuccessfully to put it into practice, his one fear was that he might hear some fresh precept.' I shall not add to their embarassment, for although what I have to say has some relation to economic policy, the precepts that I have to expound are mellow with great age.

The presumption that once a country has transformed itself into a modern industrial society, its continued material progress is assured (in the absence of a political cataclysm) seems to have long survived the loss of belief in progress in general, and until recently it appeared to be undisturbed by all the disorders of our time. We might not be prepared to go quite so far as Mill who declared in 1848: 'Whatever may be the other changes which the economy of society is destined to undergo, there is one actually in progress concerning which there can be no dispute . . . there is at least one progressive movement which continues with little interruption from year to year and from generation to generation; a progress in wealth; an advance in what is called material prosperity.' But it was at any rate possible for a distinguished authority in 1944, when a bleak view of our future might well have been held, to suggest, on the basis of certain assumptions which he presumably thought to be reasonable, that it would be possible for the British people in 1948 to consume 19 per cent. more goods and services than in 1938, to invest 25 per cent. more, and to produce enough exports to pay for all the necessary imports. In the event, things did not turn out quite so well; but the experience of this country in the decades before the War did not suggest that such a forecast was unduly optimistic. Economic statisticians have estimated that between 1880 and 1913 the real income of this country more than doubled and that average real wages rose by about two-fifths. Further, they tell us that although this progress was interrupted by the First World War, by 1924 real income had almost recovered to its pre-war level, while real wages were about 12 per cent. higher. We look back on the inter-war period as one of economic distress. Yet the estimates are that between 1924 and 1938 aggregate real income rose by about a quarter, and average real wages by over a fifth. All this confirms the common observation that we were a good deal better off on the eve of the Second World War than we had been before the First. To this progress, many factors had contributed, including, of course, our more favourable terms of trade. But the main cause was the rise in industrial production, which was estimated at 84 per cent. between 1907 and 1937. This was equivalent to an increase in output per wage-earner of 47 per cent. and, since the length of the normal working week had been reduced during the period, to an increase in output per man-hour of 65 per cent. Most of this advance was achieved after 1924—in the years of industrial stagnation, so-called. As was suggested of

another period that has a bad name in economic history, that from 1815 to 1850, the events which most impress themselves upon the attention of contemporaries are not always the most representative of the period. Economic progress (in the sense of a rise in production and real income per head) may sometimes make less impression on opinion than the instability which attends the advance. It was, of course, the persistence of heavy unemployment that had given the inter-war years a bad name. Since, however, substantial progress had been made in spite of that unemployment, and since it was thought that we had discovered how to avoid it in the future, there appeared to be justification for the confident extrapolations of 1944.

Yet many of the wise men of the past had, in the relatively favourable conditions of their own time, inclined to the view that material progress, like happiness, never continues long in one stay. The shadow of Malthus hovered over the economy of the early nineteenth century, and in the Victorian heyday Jevons had created some despondency by pointing to the probable effects of the disappearance of one of the main natural advantages upon which British industrial progress had depended. With the exhaustion of easily accessible coal 'the cost of fuel must rise,' he said, 'perhaps within a life-time, to a rate injurious to our manufacturing and commercial supremacy, . . . and the check to our progress must become perceptible within a century.' Nearly fifty years later Marshall, after referring to the rise in real wages during his life-time 'at a rate which [had] no parallel in the past and [might] probably have none in the future,' expressed fears for what the coming years had in store for us—fears based largely on the probable worsening of our terms of trade with primary-producing countries. This was, he said in 1907, an 'age of economic grace' which 'may run out before the end of the century.'

Some of these misgivings have been justified, although not always in the way that their authors expected. It is true that the Malthusian devil appears finally to have left our shores for the more congenial demographic climate of Eastern Asia. But we no longer enjoy superiority in natural resources; the age of dear British coal has come and we have fewer advantages than many other countries in access to supplies of alternative sources of power. As for the terms of trade, after two decades in which cheap imports had placed a cushion under our elbows, these have moved against us at a critical time, although of course it would be rash to assume that the present relationship between the prices of primary and manufactured products will persist for a very long period. These, however, have not been responsible for the most serious damage to the foundations upon which economic progress in the past was believed to rest. The breakdown in the system of multilateral trade, the disappearance of an international monetary standard, the abandonment of a freely working pricing system, the spread of monopoly both private and public, the establishment of burdensome taxation systems, all these would have seemed to the nineteenth-century observer to poison the springs of progress. Now it is believed that we can dispense with some of the props which our fathers thought to be essential to a progressive system; but many of them are still deemed to be necessary and these it is the object of policy to set up again. At the end of the war, however, it would have been very optimistic to expect any quick success in reaching this result, and at the same time it was obvious that our own relative position in the world economy had gravely deteriorated. How is it then that our faith in the continued material progress of this country was so largely unimpaired? Our hopes for the future depended, I think, upon two factors, first, the continuance of those improvements in industrial technique and organisation which have been the chief proximate cause of economic progress so far, and second, the preservation of full employment for a long time ahead. There seemed no cause for doubting that applied science would continue to yield its fruits in higher production, and with full employment firmly within our grasp, there was no reason, why we should throw away a large part of the benefit that technical progress made possible, as we had done between the wars. A state of full employment has been

maintained since the war ; but, in spite of that, our economic position has become precarious. It was natural therefore that increased attention should be focused on the former of the two factors on which our hopes rested, namely, technical progress and efficiency. This interest has been stimulated by numerous enquiries into our industries, inquiries which have brought to light serious deficiencies in some of them. These are now being underlined by the various Productivity Teams which are the fortunate beneficiaries of American instruction and American hospitality.

The conclusions that have been generally reached are that, while our industries made considerable technical advances between the wars, the advances were less rapid than those made by our competitors, and that if we are to balance our international accounts and even to maintain the present standard of living, massive improvements in technique and organisation are necessary throughout industry. One of the reasons for the backwardness of our older industries (so it is urged) is their unsatisfactory equipment. The higher capitalisation of American industry, the fact that the American worker in most trades is assisted by a greater amount of power machinery than the British, is held to be a major cause of the disparity in productivity between the two countries. The remedy, it seems, lies in heavier industrial investment. Now there is little doubt that differences in capitalisation correspond very closely to differences in productivity, both between countries and between periods. Economic progress, it has long been a truism to state, is closely correlated with the rise in capital per head. This, however, does not take us very far. Why do these differences exist? Why has Britain become inferior to some other countries in this important respect? Is it merely an injection of fresh capital into our industrial system that is required? These are fundamental questions which I should like to discuss.

In the first place, it seems to me that, in this diagnosis, attention has been confined to symptoms and that the deeper causes of economic disparities have been ignored. I am led to this view because I observe that, for several of our industries in which comparisons of productivity are unfavourable to us, the cause, or part of it, is to be found elsewhere than in a disparate growth in capital investment. For example, the steep rise in output per man-shift in the German coal-mining industry during the thirties, a rise which left the Germans far ahead of the British in this respect, was not attributable mainly to the use of more mining equipment, for the mechanisation of German mining had been almost complete before 1930. The chief reason given for the advance was the better organisation of mining operations. It was not that the industry was provided with more resources, but rather that it made better use of those which it already possessed. Again, the far higher productivity of the American building operative, when compared with the British in the erection of roughly similar buildings, does not depend on a much greater use of capital equipment. The chief causes are to be found, first, in the method by which the Americans organise the whole complex of operations between the time when the contract is let and the time when the building owner takes possession, and, second, in the different attitude of the American operative towards his work. Then, I am told of a company which operates almost identical plants in the United States and here and which gets substantially higher productivity from the American one. The recent experiments that have been made in the cotton industry show that large increases in productivity can be obtained without additional investment and merely by the reorganisation of the mills — the rearrangement of the machines and the ' re-deployment ' of the workers. These examples suggest that high productivity is often the result of the way in which the capital resources are used and is not determined solely by the quantity of them. The key, in other words, is to be found in organisation.

Even when we compare industries where the capitalisation in America is much higher than it is here, and where the productivity appears to be closely correlated with the amount of capital used, the causes of the disparity remain more interesting than the fact. In some cases,

failure in this country to adopt more capitalistic methods is apparently due to institutional arrangements. In the British cotton industry it is alleged that wage systems based on the conditions of the past, and the objections to multiple-shift working, have made the widespread use of more modern types of machines unprofitable. Sometimes the inferiority of our business organisers to the American has been given as an explanation. This is a point to which I shall return. There is, however, another type of argument which is more difficult to handle. Static analysis normally explains the differences in capitalisation and in organisation between countries in terms of the relative scarcities of the various factors of production. Where skilled labour is cheap and capital expensive, it is to be expected that firms will use less capitalistic methods than they do in countries where the reverse relationship applies. This may mean that the two countries in our comparison specialise on different qualities of the same goods; the first is likely to concern itself with high qualities and specialities and the latter with mass-produced goods. While this reasoning is satisfactory in explaining the contrast that exists at any moment, and while it also indicates that it would be dangerous for us to play the 'sedulous ape' to the Americans, it does not justify complacency, for the argument scarcely touches the dynamics of the problem. No doubt one reason why the Americans in the middle of the nineteenth century were pioneers in the adoption of the 'interchangeable method' of production in engineering was that, in the absence of a large supply of skilled labour, they had perforce to seek for means of economising its use. This meant the standardisation of products and processes and the employment of highly mechanised methods of manufacture. Once these methods had come into use, however, a chain of consequences followed. The initial standardisation of components and, to some extent, of finished products also, was brought about by the lack of skilled labour. But standardised output influenced consumers' tastes in the direction of uniformity, and a uniform demand thus provided opportunities for mass-production methods. In a particular context, it can be seen how what might have been originally regarded as a deficiency was later put to glorious gain; for when motor car production began, the existence of parts-making industries accustomed to working on the 'interchangeable principle' provided advantages to the American car manufacturers that were not present in this country. It was thus the enterprise of the Americans in overcoming an initial disadvantage which won for them benefits which time showed to be cumulative.

The conclusion is, then, that while material progress is affected by numerous circumstances, it is fertility in resource and imaginative enterprise which lie at its root. Where these qualities are present in a population and are given opportunities to exercise themselves, obstacles to progress are readily overcome. In the presence of enterprise, moreover, the capital needed for high productivity is seldom lacking. It is erroneous, in my view, to look to the side of savings for a leading cause of any deficiency in capital equipment, for enterprise generates its own savings out of the higher incomes which it makes possible. In the old civilisations of Asia, what has retarded economic progress is the lack of native entrepreneurial ability far more than a shortage of capital or skilled labour. As a result, in India and elsewhere, until very recently, the not inconsiderable savings of the wealthy have been used in the creation of consumers' capital or in loans for consumption purposes, and material progress was largely confined to those sectors of the economy in which foreign entrepreneurs participated. In the one Asiatic country where entrepreneurial skill was available (Japan), industrial progress was remarkably rapid as soon as it was allowed freedom to exercise itself, in spite of the initial shortage of capital. I suggest, therefore, that the capitalisation of industry is a function of enterprise, and that the higher productivity which the extensive use of capital makes possible, depends ultimately on the supply of entrepreneurs.

This brings me to another stage in my argument. Economic efficiency refers not merely to the relation between input and output in particular industries. It

has also to do with the way in which the whole resources of a community are distributed among the different uses to which they can be put. One reason why our income per head before the war was not far short of that of the United States, in spite of their superior productivity in manufacturing and mining industry, was that a larger proportion of our workers was found in those industries which in all countries have a relatively high productivity. This was the consequence of our having carried industrial specialisation to an extreme limit, with the result that in 1939 only 5 per cent. of our working population was engaged in agriculture, which in every country is a low productivity industry, compared with 20 per cent. in the United States. This specialisation was one of the reasons for our great material progress in the past. At present, when the change in conditions of international trade is forcing us at great cost to retrace our steps, we may wonder if this specialisation did not mean our giving too many hostages to fortune. Still, up to 1939, we reaped great advantages from it, and whether in the long run the policy will be counted wise may not be told until our story is finished.

The efficient distribution of resources is not a once-for-all operation. An economic system cannot progress unless with the inevitable changes in circumstances it alters that distribution. This is especially important for a country that has owed part of its material well-being to superiorities that time destroys, such as those that come from exhaustible natural resources, or to an early start. In this kind of adaptability Great Britain once showed great skill ; but her achievements in this respect during the last thirty or forty years have not matched those of her past, nor indeed those of other countries. Our industrial weakness during the inter-war years was demonstrated more by our failure to secure a substantial share of the new trades that rose during that time than it was by the probably inevitable decline of our older staple manufactures. Lack of resilience may be regarded as a serious cause of misgivings about the future, for economic progress is associated far more with a capacity to take the lead in new industries than it is with steady technical progress in the old. This is particularly true of a country that occupies a position like ours in the world. With superiority in natural resources gone, and with the knowledge and experience of established industrial technique widely spread among the peoples, we can hope to maintain a high rate of progress only if we show imagination in scouring out new channels.

This brings me to the proposition which lies at the heart of my theme. It is that economic progress springs less from the detailed improvements effected in the methods of established industries than from massive innovations which lead to the creation of new ones. In other words, the rise in material wealth depends not so much upon steady progress in turning out familiar things (although, of course, that is important enough) as upon the discovery of new ways of producing those things and upon finding new things to produce. If this is true (and I believe that the history of any period in which there has been a striking advance in material wealth justifies this view), then it means that of all the forces that make material progress possible industrial leadership is the most powerful. The key to progress is held by the man who is capable of effecting novel combinations of resources, often for the creation of fresh classes of goods. I do not think that anyone who studies the development of any economically progressive country over the last century can fail to agree about the major part that has been played by a comparatively small group of forceful pioneers. They have not always been entrepreneurs in the classical sense, that is to say, independent business men working with their own resources. Many of them have been salaried officials. What has distinguished them has been their capacity for imposing their authority on their organisation and of persuading others to entrust them with the resources necessary for new ventures. This type of man is the true entrepreneur, as distinct from the ordinary run of business man who treads the path beaten out for him. Professor Schumpeter has made us familiar with this distinction, which he used in expounding his theory of economic development ; but other economists—as well as most business men themselves, no

doubt,—have observed it. Marshall, for example, drew a contrast between those business men who 'have prospered by steady adherence to affairs, largely of a routine character, with but little use of the higher imagination . . . ' and the leaders 'on whose work the progress of industry mainly depends.' A recent investigator into business behaviour has laid emphasis on the special importance of the part played in industrial progress by the 'surging energy of the founder-type of personality.'

In his rôle as the chief contributor to material progress, the industrial leader is not concerned merely with meeting existing demands by fresh combinations of resources. His equally important function is the creation of new demands. 'Innovations in the economic system do not as a rule take place in such a way that, first, new wants arise spontaneously in consumers and then the productive apparatus swings round through their pressure . . . It is the producer who as a rule initiates economic change, and consumers are educated by him if necessary; they are, as it were, brought to want new things . . .' Professor Knight has expressed the same idea: 'It is common to think of the economic process as the production of goods for the satisfaction of wants. This view is deficient in two vital respects. In the first place, the economic process produces wants as well as goods to satisfy existing wants, and the amount of social energy directed to the former . . . is very large and constantly growing.' A German student of industrial organisation is following the same line of thought when he states; 'Industrial development is not merely the result of a passive adaptation to technical progress and market conditions. On the contrary, modern industry . . . actively influences markets . . . in order to mould them according to its requirements.' Thus, although as Adam Smith said: 'Consumption is the sole end and purpose of all production', large increases in consumption usually result from the initiative of producers who persuade people to want new things and who themselves act as if production were the end of economic activity. According to this view, then, the changes which contribute most to progress arise from impulse within industry rather than from impact upon it.

These industrial leaders set the pace in industry as a whole, for they draw after them the 'routineers' whose old markets are destroyed or whose methods are rendered obsolete by innovations. Those who cannot follow fall out of the race. So it may be said of the true entrepreneurs as was said, in a more elevated context, of the greatest nation of innovators in history: 'They were born into the world to take no rest themselves and to give none to others.' Whether this striving for material progress is a good thing, whether Mill was right in looking forward wistfully to the coming of a stationary State, are questions that do not concern me here. All I need say is that however much the present generation is repelled by some of the manifestations of material progress, it seems firm in its belief that higher real incomes are among the most desirable benefits to which mankind can attain. Again, no one would deny that the process of creating new wants, or of diversifying existing wants, is attended at times by waste. But whether in such a society as our own we need to charge our bureaucrats with the responsibility of protecting us against that waste and of saving us from the possible social disaster of getting what the innovators and their publicity agents make us think we want is a question which, fortunately, lies beyond the scope of this paper. I steer my discourse warily away from these speculations towards a purely economic question upon which the argument may throw some light. Theoretical economic analysis commonly proceeds on the assumption that a business, man, when acting rationally, enlarges his output to the point at which his net profits are maximised; but this assumption seems to be refuted by the common observation that the general run of business man is content to stop short of that point. His aim is rather to ensure to himself over a long period an income which enables him to maintain a customary standard. Now, in an unprogressive society, or even in a protected sector of a progressive society, he is no doubt able to follow this policy. But the intrusion of a few restless, ambitious and exceptionally capable innovators into the system forces the comfortable and

the psychologically well-adjusted out of their routine. They must become economic men or go under. The appetites which drive the innovators forward may, it is true, be quite other than the conscious desire to maximise profits ; indeed, their motives are obscure. Yet since pecuniary gain provides a clear demonstration of their success, their activities are likely to produce a result in conformity with theoretical expectations in their own case, and the result for industry as a whole (or for that part of it that is being affected by their innovations) may not be very different from what it would be if the theorists' assumptions were correct.

This will only be true, of course, in a society in which the activities of innovators are not seriously impeded. Where law and public opinion are favourable to free enterprise, the routineers must adjust themselves to change or go out of business. But whenever defensive action by the routineers is possible, they will take it, as can be seen by many examples drawn from both before and since the ' age of economic grace '. If such defensive action is supported by the State, then a powerful brake may be imposed on material progress. The action may take forms familiar to us in our own day, restrictive practices of the kind associated with cartels and trusts, product differentiation and the like. The object of these practices is not usually the exploitation of the consumer in the crude sense, but rather the provision of insurance against a sudden change of circumstance. The practices may win considerable public sympathy, for although the results of economic change are welcomed, the change itself is disturbing. Yet a society where defensive action in support of threatened interests is generally successful, is not a society that can expect great advances in material well-being. It may, therefore, be a true instinct among the Americans, who of all people are still the most avid of material progress, to see in cartels and monopolies the canker at the heart of the economic system.

If they are right, it is not mainly for the reasons given by static economic analysis. Economists usually object to departures from their ideal competitive system on the ground that imperfect competition and monopoly lead to higher prices, lower output and less employment than would exist if industries were conducted under conditions of perfect competition. Apart from the fact that perfect competition is often an unreal alternative in the actual world, it may be suggested that this conclusion is not of primary importance in the consideration of a dynamic system. The substitution of monopoly for competition may certainly involve waste; but as long as the industrial system as a whole is moving forward under the impetus of great technical discoveries applied by creative entrepreneurs, the wastes that can be detected at any moment in particular areas of the system as a result of monopoly are likely to be dwarfed by the progress elsewhere and are in any case likely to be temporary. The real danger of monopoly arises when it is used to impede change. A society that keeps the channels of innovation clear need have little fear of the waste that arises from monopoly or imperfect competition in *established* industries. It is when these channels become choked that material progress slows down. If we look back on the metal industries of the last quarter of the nineteenth century, we can see that, from the standpoint of economic progress, the significant fact was not the extent to which the output of wrought iron was then restricted by the imperfections of competition among the producers, each with his famous brand of iron, but rather the entire destruction of the older forms of production by the coming of mass-produced steel. In the same way, I suggest, material progress in so far as it depends upon a supply of cheap textile materials will be bound up less with the success or failure of official schemes in the United States and elsewhere for keeping up the price of raw cotton than with developments in the manufacture of synthetic or semi-synthetic fibres—developments that have already brought down the price of staple fibre below that of raw cotton.

This brings me back to a consideration of our own affairs. Because of our precarious international position, not merely an advance in our material prosperity, but even the preservation of our existing standards, depend upon our demonstrating

a capacity for industrial leadership. Yet, in spite of the achievements of our scientists and inventors, there can be little doubt that during the last few decades we have fallen behind other nations in successful industrial innovation. We have seen the undermining of our position in established industries by developments that have taken place abroad rather than at home. I can refer to only a few of the numerous causes that have been responsible. One of them, without doubt, has been the preoccupation of policy with the buttressing of threatened positions rather than with clearing the way for fresh enterprise. But policy reflects social opinion, and it may be contended that this has been hostile to the exercise of industrial leadership. Sixty years ago Marshall noted that while economic progress did not necessarily require the maintenance of those rights of property that lead to extreme inequalities of wealth, it did depend upon the existence of a social milieu sympathetic to the exercise of free, individual responsibility. Later, in his *Industry and Trade*, he pointed out that the United States was satisfying that condition more completely than this country : ' As arts and sciences flourish best where their followers work for the approval of brethren of the craft, ... so business flourishes most where the aim of the business man is not to shine in elegant society, but to be held in respect by those who are the best judges of his special form of strength.' ' This exclusive devotion to one pursuit ', he added, ' involves some loss of life to the individual, but the constructive economic force which it gives to America at this phase of her development is unique.' Although it can scarcely be said that the British business man to-day is distracted by a desire to shine in elegant society (for in contemporary England he would have difficulty in discovering that particular firmament), his ' special form of strength ' is not one which at present wins social approbation, and he is under even more insidious temptations to dissipate his energy. Consider the advisory committees, working parties, councils and associations, both official and unofficial, that have proliferated in recent years !

Centralised economic planning, moreover, necessarily restricts the functions of the industrial leader, for it leaves little opportunity for what has been called the very essence of free enterprise, namely ' the concentration of responsibility in its two aspects of making decisions and taking the consequences of decisions when put into effect.' Under planning the business man cannot on his own responsibility create new combinations of resources ; he becomes merely the administrator of such resources as are allotted to him for given purposes. Professor MacGregor, in his latest book, draws a distinction between policy and plan. ' A policy ', he says, ' is directive ; a plan is executive '. Applying this principle to our present arrangements, he argues that only a small part of our economy, the nationalised sector (which according to Mr. Morrison is to be only 20 per cent. of the whole), is to be planned, while the remaining 80 per cent. will be private and so subject to the method of policy. This definition may have its uses ; but it conceals the real nature of planning. To my mind the distinctive feature of planning when compared with free enterprise is that the business man is unable to create fresh combinations of resources unless he first obtains the permission of the central authority whose decision follows from the plan which it has made. In that sense our present-day economy is largely planned, although how far these conditions are to be regarded as permanent and how far as arising out of our special post-war difficulties is not easy to determine.

Now it does not follow that if the private entrepreneur in this country has had his day, the function of innovation can no longer be performed at all. An alternative agency is the State, and we are thus led to consider whether government is, or can be made, competent to discharge this function which is so essential to material progress. I have little time to give to this question, but I should like to make a few comments upon it. In some countries the government has played a very important rôle as an innovator, and through its initiative the economic life of whole nations has been transformed. But its most notable successes have been achieved in introducing into primitive societies, where experienced private entrepreneurs have been scarce, the

technique and forms of organisation already worked out in advanced countries. We cannot yet find, I think, any example of a State's having been responsible for developing successfully industries of an entirely novel kind in advanced societies, except perhaps those that serve military needs. When the government intervenes in such societies, it concerns itself with what is already established. It plans in relation to what exists, not in relation to what may be called into existence. Its tenderness for what already exists may even jeopardise the birth of new ventures, for economic advance frequently involves the destruction of established positions and the decay of industries that have become obsolete. A government can hardly be expected to take up new and untried ventures and to persist, in spite of disappointment and public criticism, in carrying them over the harassing period of experimentation until they have become transformed into new industries. The concentration of attention on established firms is damaging to economic progress, because new developments do not always, or even usually, spring from the more obvious origins. In this country it was not a firm in the major textile spinning industries, nor in the chemical industry, that built up the rayon industry, but a silk-weaving firm which happened at a critical moment to be employing an imaginative chemist in its dyeing department. It was not the great engineering firms that took up motor-car manufacture, but small firms in the bicycle, tinplate ware, electrical apparatus and sheep-shearing machinery trades. Further, in many lines of production, there seems to be no unique solution to the problem of efficient organisation. An equally good result can be achieved in several different ways, and it is difficult to believe that a government would deliberately promote a diversity of this kind, even if it went so far as to recognise that diversity is itself fertile. The immense variety in the methods by which a woollen manufacturer may achieve a given standard of efficiency was noted by the Wool Working Party. Diversity of method is even more characteristic of new industries in their early days than of old industries, and the superiority of any single method (if that ever emerges), is only brought out *ambulando*.

In the end the problem for government as for private enterprise becomes that of selecting the right person and of giving him scope for the exercise for his powers. The State may do this just as well as private enterprise for established undertakings, but it is not easy to see how it can carry out this function in the case of ventures which are in process of being born, for the men who are likely to do best in them are generally still young, unknown and untried. Any alternative to the older system must provide, to quote again from Marshall, some " reasonably efficient substitute " for the freedom which that system offered " to constructive genius to work its way to the light and to prove its existence by attempting difficult tasks on its own responsibility, and succeeding in them ; for those who have done most for the world have seldom been those whom their neighbours would have picked out as likely for the work." One must not, of course, press this argument too far. If the supply of private initiative shows signs of failing, either through a decline in energy or through the creation of an environment uncongenial to its exercise—and this may come about through changes in social policy which the public requires —then the State must step into the breach. The time has not come for a final judgment on this issue. It would be rash to condemn novel kinds of economic arrangements because they pay little regard to the conditions necessary for the successful operation of past systems, which themselves had plenty of faults. All that I feel justified in concluding is this. One of the chief agents of economic progress during the last hundred and fifty years is likely to have little scope for its activities in this country during the years ahead, and at present there is no clear indication of how an adequate substitute is to be provided. Since this country is as firmly convinced as ever of the desirability of material progress, and since that progress in my view will depend upon its success as a pioneer in producing and marketing new kinds of industrial products, there is serious cause for concern.

Apart from whether the government is competent in a technical sense to exercise

industrial initiative, there remains the question whether the society whose resources it has taken upon itself to administer really wishes for innovation, not merely in the sense of being prepared to enjoy its fruits, but also in the sense of accepting the conditions for their harvesting. The question which Berkeley posed two centuries ago is highly relevant to government planning in a democratic society : ' Whether it be not the Opinion or Will of the People, exciting them to Industry, that truly enricheth a Nation ? ' An investigation of the ultimate causes that produce great outbursts of energy (as distinct from the instruments which they use), and a discussion of the conditions which make those outbursts creative, and not destructive of the society in which they occur, would take us far beyond the subjects with which economists usually concern themselves. I shall content myself with a few reflections. Great industrial changes mean the disruption of older ways of life and are not always compatible with social peace. As Knight says : ' The more vital problems are not problems of economy, but of maintaining social unity in the face of economic interests.' Economic progress thus depends upon morale, which permits a reconciliation of innovation and social stability. As to the forces that have driven men to the furious pursuit of material progress, they lie, in the main, beyond the realm of cool, economic calculation ; although calculation may be their tool. The investigators of American economic superiority, after listing the technical and administrative causes, find an unexplained residūum which they attribute to ' the American way of life.' The Productivity Teams, for instance, make much of this in their Reports. The inference that British industry is supposed to draw from this explanation is not always clear ; but that ' the American way of life ' supplies much of the driving force to the American economic machine cannot be gainsaid. To go further afield, soon after I interested myself in the economic affairs of the Japanese, I noted Marshall's comment on their extraordinarily rapid economic development after centuries of stagnation ; ' Their quick rise to power supports the suggestion made by the history of past times that some touch of idealism, religious, patriotic or artistic, can generally be detected at the root of any great outburst of practical energy.' In our own early industrial development, the relationship that existed between Nonconformity and the rise of capitalist industry was once a commonplace in historical writings. In that connection, since I am speaking in Birmingham, may I be allowed to quote a passage which many years ago I gleaned from the records of one of the City's own great innovators, the firm of Boulton and Watt ? This quotation is from a letter written in 1795 by an ' adventurer ' in the Cornish mines, Richard Mitchell by name : ' I will venture to assert that, of all men for the last five hundred years, no two men have been of so much use . . . to the county as the Reverend J. Wesley with respect to Religion, and Mr. James Watt with his automatical machineries. Mr. Wesley . . . taught the vulgar to be sober, honest and industrious . . . He not only taught them to live well, but to do well ; therefore, he has now his reward in Heaven. Mr. Watt, after Mr. Wesley had taught them to be industrious, within the last twenty years has taught and helped them to get at the vast treasures which Providence till then had hidden in the bowels of their county, by his fire engines for working deep mines, without which thousands on thousands of pounds of tin and copper must have remained there forever.' The diversity of inspiration is striking ; the American way of life, Emperor-Worship, John Wesley. Fortunately, an economist is not rquired to probe into these mysteries. He notes their existence, and for the rest he must content himself with expressing in his own jargon the substance of the familiar (and it is to be hoped, complimentary) lines :

' Time and the ocean and some fostering star
In high cabal have made us what we are.'

A CHAPTER IN BRIDGE ENGINEERING A CENTURY AGO

Address by

PROF. ANDREW ROBERTSON, F.R.S.

PRESIDENT OF SECTION G

OF the sections of the British Association the one concerned with engineering must surely have a special appeal to Birmingham. From its beginnings the district has been closely associated with metal working in various forms from swords, jewellery and toys to guns, cars and aeroplanes. Indeed, one may with some justice attribute a considerable share in the development of the industrial revolution to a prominent citizen of Birmingham. It was Matthew Boulton who provided the facilities for James Watt to develop his steam engine which was the greatest single influence in that revolution. Perhaps his share in the work may be realised better to-day than it has been in the past, for problems of management and the efficient organisation of industry, about which we hear so much to-day, were his special province. As long ago as 1774 his factory employed 1,000 workpeople and his organising ability had made the Soho Foundry one of the best schools of skilled industry in the world. Since his time the advances in all forms of metal working have been spectacular and among the 1,500 different trades carried on in the Birmingham area it still holds a prominent place.

To one like myself whose professional interests are intimately associated with the Institution of Mechanical Engineers, Birmingham has a special appeal since it was here that the Institution was founded a little over one hundred years ago. It is said that the first suggestions for an institution were made at a casual gathering of railway engineers sheltering from the rain in a railway hut near the Lickey Incline in 1846. Conversation turned on the refusal of the Institution of Civil Engineers to admit George Stephenson without the production of ' a probationary essay as a proof of his capacity as an engineer.' After a preliminary testing of opinion a meeting was held in the Grand Hotel, Birmingham on October 7, 1846. This was followed by another in January 1847, at which the Institution was founded with George Stephenson as the first President and here it had its home until 1877 when, on a resolution of the general meeting, it was decided to move the business and the house to London.

On glancing at the subjects chosen by Presidents of Section G in recent years, I came to the conclusion that those on which I might with some vanity have presumed to address you, had been covered. I therefore thought that it might not come amiss if I spent the time in recalling an event which made a considerable stir in the engineering world one hundred years ago, i.e. The Britannia Tubular Bridge which was opened on March 18, 1850. The construction of this bridge was preceded by a notable series of experiments and it represented a remarkable instance of that combination of engineer, constructor and mathematician which has proved so fruitful in engineering. I hope no apology is needed for raking up the past. Indeed, I think that we engineers might well take a leaf from the book of our science colleagues and devote a little time in our courses to the main historical developments of our subject.

It is recorded of Henry Ford that he thought history was bunk and that he had no use for keeping records of experiments. There is an element of truth in the contention that a man who is chock-a-block with knowledge of what has been done is sometimes more likely to find reasons for not

making new experiments than he is to devise them. But the real interest is not so much in the actual records as in the insight one gains of the way the men tackled their problems. The engineer is always on the lookout for means of improving his work and I cannot but think that some knowledge of the way our predecessors utilised a new material can be productive of anything but good. The courage, skill and audacity which are required to-day are exactly the same as were needed one hundred years ago when it became possible, through the exertions of a few men, to utilise the relatively new material—wrought iron. And it is good, too, to be reminded that as good fish came out of the sea as are now in it.

I propose then to take you back one hundred years or so and look at bridge engineering and leave it to Mr. Roberts and Mr. Russell to say something of the problems of construction which arise to-day and may arise to-morrow.

The enormous success of the steam locomotive and the remarkable increase in the number of railways necessitated the construction of a vast number of bridges. Robert Stephenson estimated that up to 1856 at least 25,000 railway bridges must have been built in the United Kingdom. The early bridges followed traditional practice and were built of stone, brick or wood. The credit for erecting the first iron bridge belongs to this country. This was the cast-iron semicircular arch of 100 feet span built over the Severn at Coalbrookdale in 1779. The many short bridges required for the railways were generally cast-iron beams, and the researches of Hodgkinson and Fairbairn, many of which were reported to the British Association, had provided a satisfactory basis for design. Single beams were sometimes 50 to 60 feet long ; larger spans were obtained by bolting together several sections and were usually used in circumstances in which arches were inadmissable. In judging the suitability of a design the engineers of the day frequently made a full-size test to destruction and aimed at an ultimate strength from three to six times the working load, depending on conditions. Some had proof tests of twice the working load applied to each member but this practice was not universally approved and some relied on good inspection at the foundry. Perhaps as good an example as any is that adopted by Telford for the links of his Menai Suspension Bridge in 1826. The complete links generally failed under a load of 87¾ tons. Telford insisted on every link being tested with 35 tons (11 tons per sq. inch.) and whilst the load was on, the link was occasionally hit with a hammer.

The first entirely wrought-iron bridge (other than suspension) was one of 31½ feet span built in 1841 by A. Thompson to carry a roadway over the Pollock and Govan Railway. In 1846 William Fairbairn built one of 60 feet span for the Blackburn and Bolton railway.

The engineers of the time were alive to the advantages to be gained by using wrought iron in tension and cast iron in compression, but some were suspicious. The failure of one such bridge over the Dee in 1847 led to the appointment of a Royal Commission whose Report I shall have occasion to refer to later.

The shipbuilding industry had been more alive to the use of wrought iron than the bridge builders, for barges had been built for the Monkland Canal in 1818. By 1832 Messrs. Laird were bold enough to build iron vessels for ocean navigation. The finest example of this period was the *Great Britain* built in 1843 by Paterson in Bristol and designed by Brunel. Her career was interrupted by stranding in Dundrum Bay and the way in which she stood up to the buffeting of the waves for a whole winter did more than anything else to justify wrought iron as a material for ships. By the efforts of Brunel himself she was finally refloated and only a few plates were found to be damaged. The last I heard of her, only a few years ago, she was doing duty as a coal hulk in the Falklands.

In order to complete the Chester and Holyhead railway it was necessary to cross the Conway River and the Menai Straits. When the bill was presented to Parliament in 1843–4 the chief engineering work was the Conway Bridge, since it was proposed to use one of the roadways of Telford's Suspension Bridge for the conveyance of carriages and to have a locomotive ready on the other side to take the reassembled train on to Holyhead. The Commissioners of Woods and Forests refused to allow this

as a permanency and the company was driven to propose an independent bridge. The Bill was accordingly passed with a gap of five miles at this point. The directors instructed Robert Stephenson, who had become their engineer, to select a suitable place for the bridge. He decided on a site about a mile to the west of Telford's bridge because of the presence of the Britannia Rock (hence the name of the bridge), in the middle of the Straits. The first project was two cast-iron arches each of 350 feet span with the road 105 feet above high water at spring tides. The cast-iron arch itself would have been a very big increase on anything hitherto attempted. Some details of the largest arches in existence at the time are given in the following table.

	Material	Span of Centre Arch	Rise of Arch at Centre	Date of Completion	Engineer
		Feet	Feet		
Sunderland over the Wear .	Cast Iron	240	30	1796	Wilson
Southwark Bridge . .	Cast Iron	240	24	1818	Rennie
Dee Bridge, Chester . .	Sandstone Arch	200	42	1833	Hartley
Pont du Carrousel over the Seine	Cast Iron	187	15½	1836	Polonceau
London Bridge . . .	Granite	152	29½	1831	Rennie
Maidenhead Bridge over the Thames on Great Western Railway	Brick	128	24¼	1835	Brunel

The new bridge called for a span of 350 feet and entirely novel methods would have to be designed for building it as the Straits could not be closed against navigation. Stephenson proposed to build the arch without any centering by adopting a proposal originally made by Sir Isambard Brunel. This consisted of progressively placing equal voussoirs on opposite sides of the pier and holding them in position by tie rods. When the type of bridge which Stephenson proposed became known, strenuous opposition from those interested in the navigation of the Straits was experienced. The Admiralty sent down a commission composed of Sir John Rennie, J. M. Rendel, and Captain Vidal, R.N. The commission decided that an arch was unacceptable and demanded a clear passage throughout the whole span of 100 feet above high-water level.

Anticipating the findings of the Commission, Stephenson began to consider other possibilities. A suspension bridge of the type then in use was out of the question. He was aware of the improved system of trussing by which Rendel had dealt successfully with a road bridge at Montrose which had failed on two previous occasions. He also knew of the Stockton and Darlington bridge, the roadway of which rose in waves 3 feet high when a train was run on to it. He was looking for a stiff platform and for a way of making this work in harness with the characteristic flexibility of the suspension bridge. The form which the bridge was taking in his thinking was that of a huge wrought-iron rectangular tube through which the train would run and which would be supported in some way by chains. The rectangular form he later thought was objectionable and he turned to circular and elliptical tubes.

In March 1845 he instructed his assistants Berkley and Marshall to prepare drawings embodying this tubular idea with the thickness at the top and bottom double that of the sides. The calculations convinced him that he was justified in informing his directors that the bridge was possible and that he could face the forthcoming Parliamentary enquiry with some confidence.

Sometime in April 1845 Stephenson was

explaining his proposals to his father George who was always interested in his work ; (in fact Robert always spoke of his own work as ' ours ' even though the father had no responsibility for it). It so happened that William Fairbairn called at the time when the discussion was in progress and the scheme was explained to him. Fairbairn at once expressed his confidence in the project and from his shipbuilding experience gave many instances of the remarkable strength of iron ships. Fairbairn was by this time an experienced shipbuilder and researcher into the strength of materials and much of his work was reported to the British Association. One example was particularly striking. In launching the *Prince of Wales* there was an accident so that her bow became jammed on a wharf and on the tide receding she was left with 110 feet of daylight showing between the wharf and the point of contact of the water and the ship. These favourable reports were fully confirmed by the experience of John Laird, the shipbuilder.

At the Parliamentary enquiry Stephenson stated his position as follows, on May 6, 1845.

' Perhaps I may at once explain to the Committee the idea I have adopted. I conceive a tube : supposing a wrought iron tube to extend across the Straits ; that tube to have, we will say, 25 feet diameter to hold a line of railway, and the line of railway would run inside it. In addition to that, we should have to erect a *chain platform* for the purpose of building. Then the question would arise, *whether the chains would be allowed to remain*, or whether they would be taken down. My own opinion is, that a tube of wrought iron would possess sufficient strength and rigidity to support a railway train. I am instituting a series of experiments in conjunction with Mr. Fairbairn of Manchester ; he is already in possession of experiments with respect to iron ships, which places the thing beyond doubt. He has ascertained that a vessel of 250 feet in length, supported at the ends, will not yield with all the machinery in the middle. There are several cases which I could quote of iron vessels having been stranded with the steam-engines in the centre, without injuring the construction of the vessel.'

Stephenson's attitude over the retention of the chains seems to have been influenced by the opinion of General Pasley (then Inspector General of Railways) to whom he had explained his general idea early in April. Pasley urged that there was no object to be served in taking them down and prudence dictated their retention.

Now, if we recall that the largest wrought-iron girder bridge at the time had a span of 31½ feet and the experience of wrought iron was largely confined to a few small ships, we have formed some idea of the courage of these two men. They realised, however, that in a project of this magnitude involving so much capital and the lives of so many people, they must go cautiously, and as the scheme was entirely outside all experience, they decided to carry out some experiments to enable them to arrive at a suitable design. These were placed in the capable hands of Fairbairn who had much experience of iron work and had in his workshop all the facilities for constructing the specimens.

The experiments were first directed to discovering the way in which a tube of wrought iron behaved when subjected to a bending moment. Tubes of 12 to 24 inch diameter and 15 to 31 feet long were made by rolling sheet iron 0·037 to 0·135 inch thick and riveting. They were tested under a central load which was applied by dead weight through a bolt resting on about 8 inches of hard wood. The hole through which the bolt passed was reinforced. The experiments brought into prominence the phenomena of wrinkling when the portion of the tube under compression failed by crumpling up into folds. Actually only two of the tubes failed on the compression side at a stress of about 15 tons per sq. inch and all the others failed on the tension side usually at or near the riveting. In the next series of five tubes which were elliptical in section, three failed on the compression side at appreciably higher stresses. Fairbairn also tested some rectangular tubes and one which had longitudinal circular corrugations on the compression side, which virtually made it a row of tubes. The latter gave a very good result and the others indicated the dependence of the strength on the thickness.

Fairbairn now recognised the need of a

mathematician to deduce, from the experiments on the small scale, formulæ and modes of calculation applicable to a larger size. With the concurrence of Stephenson he invited Eaton Hodgkinson who had been associated with him in investigations on the strength of iron and who had been responsible for placing the design of cast-iron beam on a satisfactory basis. Perhaps the most interesting series of tests is one instituted by Hodgkinson to ascertain how far the failure stress depended on the thickness alone, the other dimension remaining the same, and to obtain the strength of tubes similar in form but varying in size.

With a span of 30 feet the crushing stress at failure of a beam 24 inches deep and 16 inches wide was 19·17 tons per sq. inch for a 0·525 inch plate; 14·47 tons per sq. inch for a 0·272 inch plate; and 7·74 tons per sq. inch for a 0·124 inch plate.

When every dimension was quartered the one with 0·132 inch plate gave 23·17 tons per sq. inch and that with 0·065 inch plate 15·31 tons per sq. inch. When the scale was again halved so that the span was now 3 feet 8 inches and the section 3 × 2 inches the 0·016 plate gave a stress of 24·56 tons per sq. inch and the 0·03 inch plate gave a stress of 13·42 tons per sq. inch.

These results showed that, provided the thickness was suitably adjusted to the dimensions, a rectangular tube could be constructed which would sustain a high stress. Having obtained this information Stephenson, on February 8, 1846, reported to his directors that he was satisfied that the adoption of a wrought-iron tube was the most efficient as well as the most economical construction for the bridge. He was satisfied that the only important question remaining to be determined was the absolute ultimate strength of a tube of given dimensions. In order to assess this accurately he approved of further experiments by Mr. Hodgkinson. To his report Stephenson appended separate reports from Fairbairn and Hodkginson. It is interesting to note that whilst Stephenson and Hodgkinson were in favour of the retention of the chains which it was then assumed would be necessary for erection, Fairbairn advocated their rejection. In fact he says ' it should be a huge sheet iron hollow girder of sufficient strength and stiffness to sustain the weight and provided the parts are well proportioned and the plates properly riveted you may strip off the chains, and leave it as a useful monument of the enterprise and energy of the age in which it was constructed.' Hodgkinson on the other hand begs to recommend that suspension chains be employed as an auxiliary, otherwise great thickness of metal would be required to produce adequate stiffness and strength.

You may be interested in the comment of the historian of the bridge, Edwin Clark, who, after referring to the difficulty he has in retracing the steps which led up to the successful conclusion, says : ' We cannot but participate with Mr. Stephenson in his unshaken confidence in the justice of the principles with which he started on this enquiry, not confirmed by the test of experience, nor can we fail to admire the zeal with which Mr. Fairbairn anticipates, (May 1845) the result of the important investigations in which he was engaged, whilst his sanguine conclusions contrast forcibly with the abstract and minute deductions, the sceptical doubts and fears of the exact mathematician.'

The necessity for further experiment was obvious even without the dismal prophecies of disaster which came from some quarters. Hodgkinson suggested the construction of a model tube one-sixth full size. The sides were to be just sufficiently strong to preserve the form of the tube and the top and bottom were to be chosen so that they failed simultaneously. He also suggested that the top should be replaced by a tube and that a series of experiments should be made on the resistance of wrought iron to crushing.

To all this Stephenson agreed and, in May 1845, instructed Fairbairn to make the tube. The model was completed in July and experiments began at once. The model was a pretty formidable affair, 77 feet 8 inches long for a span of 75 feet and 4 feet 6 inches deep, 2 feet 8 inches wide. The top was formed of cells $6\frac{1}{2}$ × 6 inches, horizontal plates 0·179 inch thick, and web plates 0·151 inch. The bottom was a single plate 0·179 inch thick and the sides 0·1 inch. The cells were formed with angle irons at the corners.

The whole weighed over 5 tons. The model was tested by a central dead weight of ' pigs of iron ' and boiler plate placed on a wooden platform, which was suspended from a strong wrought-iron bar which passed through the sides and rested on a stiff plate on the bottom.

In the first experiment the bottom fractured under a load of 35·5 tons.

The bottom plate was then increased in thickness over the middle 40 feet. A pucker in the sides was noticed at 25 tons and this developed and at 43·3 tons the side suddenly collapsed and the tube rolled over. The top and bottom remained perfect.

In the third experiment the sides were stiffened with internal vertical angle irons 2 feet apart, 33 of them on each side, and a braced frame inserted at each end. It was first loaded with 34·617 tons which was left on for 19 hours during which the deflection increased from 3¼ to 3½ inches. After several days rest it was again loaded and at 54·2 tons buckling of the sides became visible. At 56·295 tons the buckling had increased, the top was slightly distorted and the tube ' was complaining in all its parts.' Final failure occurred by tearing asunder at the bottom, the fracture running up a line of rivets on each side.

At the British Association meeting of 1846 (Southampton) both Fairbairn and Hodgkinson read papers which exhibited a marked divergence of opinion. Fairbairn gave details of the experiments on the large model and Hodgkinson advocated the use of circular cells as being stronger than rectangular ones. He also gave particulars of the further experiments he proposed making. There was, too, considerable controversy in the Press. Fairbairn contended that the practical difficulties of manufacture were all against circular cells and that the difference in strength did not warrant the change. Naturally, questions of construction appealed strongly to Fairbairn who in the previous May had been appointed to superintend the construction and erection of the Conway and Brittania Bridges in conjunction with Stephenson. Hodgkinson on his part was perhaps suspicious of the first preliminary design which had been completed in July and which had a top consisting of a double row of cells of ½ inch plates. The incident caused considerable annoyance to Stephenson who had not been aware that the papers were to be given.

Further experiments were carried out one of which was with the model on its side. The sixth and last was made in April 1847. According to Mr. Clark it is the most important one although it was made too late to be of much service in assisting in the design of the tubes. In this experiment the top had an area of 26·56 sq. inches, the bottom 22·45 sq. inches, and the sides 9·6 sq. inches. The ends were provided with internal frames to preserve their shape and the sides had angle irons every two feet and an arch of angle on each side. Under a load of 70 tons the sides began to buckle near the middle and at 73 tons buckles also appeared at the ends. At 79 tons the top began to show buckling at the centre and at 80 tons the buckling of the sides increased. At 86 tons failure occurred by the top crushing near the centre at a part which had previously been injured. The tube was found to be greatly strained all over, the undulations in the side being very evident from end to end.

The final result was that the cellular top of the model failed under a stress of 14·8 tons per sq. inch and the corresponding tension on the bottom was 18·6 tons per sq. inch. One other important test should be noted. On May 7, 1847, Fairbairn tested, in the works of Benjamin Hicks of Bolton, a specimen 8 feet long, 18 inches square, made of ½ inch plates and angle irons. It failed under a load of 690 tons at a stress of 13·6 tons per sq. inch.

By this time (February 1847), the general idea of the design had settled down to a single row of square cells for both top and bottom. The cellular form was necessary since ¾ inch plates were the largest procurable. The size of the cell was determined by the minimum required to accommodate the holder-up and later the painter. It is of interest to note that about this time Hodgkinson was advocating cast iron for the top. Although Stephenson attached some importance to the use of cast iron he did not favour its application for these bridges. In this, as

subsequent events were to show, he displayed admirable judgment.

The application of the tests on the model to the full scale were greatly facilitated by the relations which must exist between model and full scale as deduced by Sir G. Airey, the Astronomer Royal, who was a friend of Stephenson, and with whom he corresponded on the subject. The departure from strict similarity was mainly in the sides owing to the stiffening angles and gussets.

The final estimates were :

	Britannia (one tube)	Conway (one tube)
Span .	. 472 feet	412 feet
Top .	. 450 tons	338 tons
Sides .	. 398 ,,	286 ,,
Bottom	. 437 ,,	314 ,,
Total	1,285 ,,	938 ,,

Sectional area at centre

Top .	. 640 sq. ins.	608 sq. ins.
Sides	. 306 ,,	259 ,,
Bottom	. 561 ,,	508 ,,
Total	1,507 ,,	1,375 ,,

The final shape is very different from the one Stephenson started with which was a tube $\frac{7}{8}$ inch thick at the top and bottom and $\frac{1}{2}$ inch at the sides and estimated to weigh about 450 tons.

Whilst the experiments were in progress consideration had been given to the very important problem of the method of erection since this has a powerful influence on both the method of construction and on the design itself. We have seen that in Stephenson's original conception chains would have been required for erection. He thought of building a railway on the platform suspended from the chains and in order to avoid changes of shape he proposed to cover the whole span with a line of wagons loaded to give the same weight per foot run as the tube. The tube would then be built on rails and as the completed tube was pushed forward the wagons would be pushed off at the other end. The idea of lifting the tube bodily had occurred to him but he was afraid of the risks which would have to be run during the navigation of the pontoon carrying the tube to the site. His assistant, Edwin Clark, had been much impressed by the ease with which he had seen a tank raised with screw jacks and followed up with packing step by step. He discussed it with Fairbairn's assistant Blair and Fairbairn himself, and together they recommended it to Stephenson, who finally approved the plan.

It was further decided that in the Britannia Bridge advantage should be taken of the several spans to secure the benefit of continuity. All these contingencies had to be considered in the final design.

On account of the weight of the top and the relatively flabby sides it was also decided that part of the weight should be taken by spade bolts attached to the side and depending from cross girders in the tower. To meet these various conditions an elaborate cast-iron frame was fitted at each end, and the bottom cells were fitted with castings to distribute the load between the upper and lower plates of the cells. The inside frames stiffened the ends and to them could be attached horizontal beams from which the lifting chains could take their load. The expansion of the tube was provided for by a series of rollers on the bottom of the tube and a series of balls under the member to which the spade bolts were attached. In the Britannia Bridge the fixed point was at the rock.

A few points relating to the actual construction of this ' Boilermakers Bridge ' as it has been designated may be of interest. Several innovations in current practice were introduced. The usual hammers, $2\frac{1}{2}$ to 4 lb., used in boiler work were found to be too light for the 1 inch rivets so 7 lb. ones were used with a 60 lb. one for holding up, but not without some opposition. Machine riveting was also tried and gave very good results. The machine had a steam cylinder of 48 inch diameter and worked at 40 lb. per sq. inch. The men, however, were all advocates of hand riveting and although three machines were erected for the Britannia their use was not persisted in and the whole of this bridge was hand riveted.

Altogether some 2,000 tons of cast iron was used in the construction of the Britannia Bridge of which about half was actually united with the wrought iron of the tube. To assist the arrangement of

the large mass of cast iron in the towers a model, to a scale of ½ inch to a foot, of the whole of the apparatus was prepared by Edwin Clark in Great George Street under Stephenson's direction. The model was subsequently sent to Manchester and with Fairbairn's assistance the working drawings were completed.

As the whole scheme had excited considerable controversy it was decided to test the first (Conway) tube. Some had predicted a deflection of two or three feet and a number of authorities had affirmed that the tube could not sustain its own weight. One of them declared that ' an iron tube 450 feet long to carry a train of carriages 150 tons in weight at 40 miles per hour is a chimera. It is said that the strength and form of the tube have been satisfactorily determined ; that the stone piers are to be commenced immediately ; and that the whole work will be completed in two years. Almost wherever the statement was read there was a startling apprehension that a great and expensive work, involving the reputation of many scientific men, and perilling the public safety, was to be undertaken on very slight and imperfect data.'

Scientific men generally either remained neutral or ominously shook their heads and hoped (let *us* hope) for the best.

' I have no doubt ' said one of our most eminent philosophers, ' that the strength of the tubular bridge, when new, supposing ordinary care to be used in most parts, and considerable attention given to accuracy in those parts which are to resist crushing, will be more than abundantly sufficient. I think also the permanent strength of the bridge may be quite sufficient ; but I am not so decided upon this. These opinions would be scarcely affected by a moderate alteration in the thickness of the plates. But these considerations apply only to the weights placed quietly upon the bridge ; and I should not think myself justified in expressing any opinion which could be supposed to apply as to the capability of the bridge to resist the sudden introduction of a weight at railway speed. On this subject experimental information is wanting to me. All that I can tell is that a stiff structure would be likely to suffer more from it than a flexible one. My opinion also depends entirely upon the assumed accuracy of those parts which are to resist compression. If, in the tubes intended for this purpose, there are no transversal stops or frames, their strength for resisting thrust will be very greatly a matter of accident. My notion would have been to rely upon tube structure for stiffness, and upon something else—as chain suspension, for absolute permanent strength.' In the face of such criticism it was undoubtedly advisable to have a test.

In order to deal with the big loads required for the test, someone proposed to block up the ends and run in water to the prescribed height. This was not entertained and finally ballast wagons were loaded up with iron plates and run into the tube. The Conway tube was built with a camber of about $6\frac{9}{10}$ inches and under its own weight deflected $7\frac{7}{8}$ inches and under 300 tons spread over about 300 feet, $10\frac{15}{16}$ inches.

After the test was completed docks were made under the tube and the pontoons introduced. These were designed to take 460 tons each and there were six of them for the Conway tube. They were provided with valves for admitting water into them, and pumps for evacuation. In the case of the Britannia Bridge, in order to devise a procedure for the actual passage from the shore to the site a model one-sixth full size was made and Stephenson had the help of Brunel and Captain Claxton, R.N., who had been associated with Brunel in the salving of the *Great Britain*. There were several accidents in connection with these operations and with the Conway tube it almost looked as if there would be a complete failure. Unfortunately, in approaching its ultimate position one of the pontoons which had been slightly slued by the stream fouled on the rock at the Conway end of the tube ; it was however blocked up on a temporary shelf that had fortunately been prepared in case of such an accident. It was some days before it was possible to secure the pontoons beneath the tube and so raise it the necessary few inches.

The method of raising the tube is interesting and consisted in having large presses in the towers with long suspension links attached to the tube. The ends were raised 6 feet at a time and followed up by wood packing over the middle half.

Before another lift was made the sides were filled up with brickwork in cement and the timber removed and the whole filled up. When the Britannia tube had been raised about 24 feet a serious accident occurred, for the bottom was blown out of the cylinder and the crosshead descended with a fearful crash on to the tube. The end of the tube fell through 8 or 9 inches, a fairly good test itself. In this way the separate tubes of the Britannia Bridge were raised to their positions and in joining up with the centre portions over the pier it was arranged to join up so as to get the benefit of a continuous beam.

The first Conway tube was opened for traffic on May 1, 1848 and the first Britannia one on March 18, 1850. I am informed that there has been no constructional alteration made since that date ; it is as Stephenson left it. It is regrettable that just before the opening of the Conway tube differences arose between Fairbairn and Stephenson about the precise position which the former occupied and on May 22, 1848, he sent in his resignation which was accepted. At this distance of time there can be no profit in attempting to assess their respective responsibilities for the bridge. I prefer to quote the paragraph in Fleeming Jenkin's article on Bridges in the Ninth Edition of the *Encyclopædia Britannica* : ' Mr. Hodgkinson had, perhaps, the smallest part in the design, but the shares of Fairbairn and Stephenson respectively cannot be very rigorously apportioned ; nor is this now of much consequence. Both engineers were men of extraordinary merit, and co-operated in producing the great revolution in practice which has led to the adoption of the wrought iron girder as the most common type of bridge.'

After the design was settled and during the whole time the bridge was being built, Hodgkinson continued his experiments and accumulated a considerable mass of data which was reported to the Royal Commission on the use of wrought iron in railway bridges, and is also published in Clark's volumes on the Bridge.

It is not without interest that in the development of metal construction for aircraft during and after the first World War problems precisely similar to those which confronted Stephenson were met and solved in precisely the same way. The successful designs of spars generally involved corrugated thin sheets and bear a close resemblance to one of the early designs. It is interesting also to note that in later developments it was found advantageous to do away with circular corrugations and use rectangular ones and for precisely the same reason that led Stephenson to them ; ease of construction and facility of attachment of other members.

The success of the bridge was largely instrumental in extending the use of wrought iron in bridge work. Notable examples in which the information obtained for the Britannia Bridge was utilised are the bridges of Brunel at Chepstow, 1852, and Saltash, 1859. Indeed, Brunel built several other bridges in which he used a tube for the compression and sometimes a triangle for the tension side. One, a swing bridge over the Brunel lock at Bristol, has been in continuous use since 1849. Perhaps the greatest compliment paid to the bridge was the adoption by Brunel of the basic idea for his leviathan the *Great Eastern* in 1854. The construction finally adopted consisted of a cellular upper deck and double bottom rising to 34 feet above the keel at which height a strong iron deck was constructed. (She was virtually one ship inside another except at the stern.) The upper cellular deck was 58 feet above the keel and between it and the lower deck two other light decks were built for cabins, etc. For 350 feet of the length amidships in the space occupied by engines, boilers and bunkers, two strong longitudinal bulkheads extended from the cellular bottom to the upper deck. The main frames of the double bottom were placed 30 inches apart below the bilges and 5 feet apart above them. Similar longitudinals stiffened the deck. The transverse bulkheads were spaced 60 feet apart and no opening in them below the second deck. In addition, there were partial bulkheads every 20 feet. There were no closely spaced frames of the usual kind. The upper deck ran flush and clear from stem to stern for a breadth of 20 feet on either side of the skylight and hatchways. By the watertight bulkheads and longitudinal frames the cellular double bottom was divided into 50 compartments. Indeed, in regard to subdivision the *Great*

Eastern compares favourably not only with any ship of her time but is in advance of most existing ships to-day. As to strength there is no doubt about her success for she was severely tested in the 32 years she remained afloat. She was repeatedly beached on a gridiron. On one of her transatlantic voyages she ran against a pointed rock but the voyage continued without hindrance. It was afterwards found that holes of a combined length of 100 feet had been torn in her outer skin but thanks to the double bottom she made no water and her passengers were unaware of the incident.

After a close examination of the *Great Eastern*, Sir William White declared (1903) that the weight of iron in the structure of the ship is considerably less than that of steel-built ships of approximately the same dimensions and of the most recent construction. It should be remembered that the *Great Eastern* was built of iron plates 10 feet long, 33 inches wide, $\frac{3}{4}$ inch thick, weighing $7\frac{1}{2}$ cwt., of a tenacity say 24 tons per sq. inch; whereas a ship like the *Oceanic* had steel plates 28 feet long, $4\frac{1}{2}$ inches wide, 1 inch to $1\frac{3}{8}$ inch thick weighing 2 to $3\frac{1}{4}$ tons with an ultimate in tension of 30 to 32 tons per sq. inch.

Interest in bridge design was intensified by the failure, (Robert Stephenson's first) of one of the spans of a bridge over the River Dee on May 24, 1847. This was a built-up cast-iron girder of 98 feet span trussed with wrought-iron tension bars. Each girder consisted of three separate castings 3 feet 9 inches deep. The tension bars were placed on each side and formed a chain of three long links, the middle one horizontal and placed about the level of the bottom of the girder and the two outside links rising up obliquely towards the ends which stood about 4 feet above the top of the girder, being bolted to large bosses projecting upwards from the top of the girder. The design was probably made in 1845 and the bridge was passed by the Government inspector on October 20, 1846.

Nothing occurred to attract attention up to the day of the accident. Alarmed by the fate of a G.W.R. bridge which had been burnt down by cinders from an engine, the engineer had 18 tons of broken stone laid on the wooden platform. This was done in the afternoon and the first train to use the bridge after this had been done was the fatal one. One of the girders broke in three pieces. The engine which ran on beyond the fracture was the only vehicle that remained on the line. Five people were killed and all the occupants except the driver suffered injuries great or small. The Government referees appointed to investigate the accident condemned the bridge because of the uncertainty of the proper action between the cast-iron members and the wrought-iron tension rods. Their report was communicated to the coroner's jury on the last day of their sitting and seems to have guided them in their decision. To their verdict of accidental death the jury added that they were of the opinion that the girder broke from being made insufficiently strong to bear the pressure of quick trains passing over it; also that they considered the remainder of the bridge unsafe, and finally that for the security of the public they recommended a Government enquiry as to the safety of such bridges in general. The railway commissioners, doubtless influenced by these suggestions, passed a minute calling the attention of the Government to the uncertainty which existed about the conditions to be complied with in employing iron in engineering works and in particular to bridges, which had to be traversed by loads of extraordinary weight at great velocities; that they considered it desirable that the subject should be thoroughly investigated by a commission of scientific men and practical engineers appointed by the Government, who should be requested to arrive at such principles and to form such rules as might enable the engineer and mechanic to apply the metal with confidence in their respective spheres.

The commission was appointed by Royal Warrant on August 27, 1847 and consisted of Lord Wrottesley, Professor Willis, (Jacksonian Professor at Cambridge) Captain James (R.E.), Mr. (afterwards Sir) William Cubitt and Mr. Eaton Hodgkinson with Captain Douglas Galton (R.E.), as secretary.

The important clause in the terms of reference of the commission is that ' they shall endeavour to ascertain such principles and form such rules as may enable the

engineer and mechanic, in their respective spheres, to apply the metal with confidence, and shall illustrate by theory and experiment the action which takes place under varying circumstances in iron railway bridges which have been constructed.'

Many of the prominent engineers of the time viewed with alarm this clause lest it should lead to rigid 'rules to be observed' laid down by the Government. Brunel, most independent of men, was very outspoken and called it the commission 'for stopping further improvements in bridge building.' Brunel had expressed, both publicly and privately, his doubts about the advantage of such an enquiry and he had thought that since the members of the commission had full knowledge of his attitude he might have been passed over and not invited to assist in the proceedings. However, he was asked by the commission for his 'opinion upon the enquiry referred to them' and he replied in a long letter stating his views and recording his protest and concluding with an expression of his willingness to attend and afford any information in his power. I cannot forbear quoting a part of his letter: 'If the commission is to enquire into the condition "to be observed," it is to be presumed that they will give the results of their enquiries, or in other words, that they will lay down, or at least suggest, "rules" and conditions to be (hereafter) observed in the construction of bridges, or, in other words, embarrass and shackle the progress of improvement to-morrow by recording and registering as law the prejudices or errors of to-day. Nothing, I believe, has tended more to distinguish advantageously the profession of engineering in England and America, nothing has conduced more to the great advance made in our profession and to our pre-eminence in the real practical application of the science than the absence of all *règles de l'art*, a term which I fear is now going to be translated into English by the words "conditions to be observed." No man, however bred or however high he may stand in his profession, can resist the benumbing effect of rules laid down by authority. Occupied as leading men are they could not afford the time, or trouble, or responsibility of constantly fighting against them—they would be compelled to abandon all idea of improving upon them; while incompetent men might commit the grossest blunder provided they followed the rules. For, in the simplest branch of construction, rules may be followed literally without any security as to the result.

'In the quality of the material, the workmanship, or the mode of manufacture, and in the application of it, there is every imaginable variety, there is room for almost any imaginable degree or nature of improvement; and unless the Commissioners are endowed with prophetic powers, it is impossible that they can now foresee what may be the result of changes in any one of these conditions.

'What rules or "conditions to be observed" could be drawn up now that would not become, not merely worthless, but totally erroneous and misleading, under such improved circumstances? But above all, I fear—nay, I feel convinced—that any attempt to establish any rules, any publication of opinions which may create or guide public prejudice, any suggestions coming from authority, must close the door to improvement in any direction but that pointed out by the commissioners, and must tend to lead and direct, and therefore to control, and to limit, the number of the roads now open for advance. Devoted as I am to my profession, I see with fear and regret that this tendency to legislate and to rule, which is the fashion of the day, is flowing in our direction.'

Robert Stephenson in his evidence also expressed his belief that the commission would have infinite difficulty in laying down anything like rules. 'I cannot' he said 'conceive myself going on successfully and being tied down to preconceived rules or limitation as to the extent to which cast iron should be used and the forms that it should be used in. I think a collection of facts and observations would be most valuable but if you attempt to draw conclusions from those facts and confine engineers even in a limited way to those conclusions, I am quite sure that it will tend to hamper the profession very much.'

Besides collecting a mass of information about the properties of cast iron and the current practice among engineers, the

commission directed its attention to two principal enquiries :

(i) Whether the substance of metal which has been exposed for a long period to percussions and vibrations undergoes any change in the arrangement of its particles by which it has become weakened.

(ii) What are the mechanical effects of percussions and of the passage of heavy bodies in deflecting and fracturing the bars and beams upon which they are made to act ?

Practically the whole of their work was concerned with cast iron and they deliberately refrained from expressing any opinion upon wrought-iron bridges as ' they were of such recent introduction that no experience had yet been acquired of their powers to resist the various actions of sudden changes of temperature, vibrations, and other causes of deterioration.' They did bear testimony to the patient care and scientific manner in which the forms and proportions of the great tubes of the Conway and Britannia bridges have been elaborated, for full particulars had been supplied by Stephenson and Clark.

In the course of his evidence Fairbairn gave the result of several time tests on cast-iron 1 inch square bars under a central load on a span of 4 feet 6 inches, some of which he had previously reported to the British Association. One bar broke under a load of 448 lb. after 37 days, another sustained this load from June 23, 1838 to April 19, 1842, nearly four years without any appreciable change in deflection.

The commission itself instituted a series of valuable experiments which broke entirely new ground in experimental work on materials. The principal were :

1. *Repeated Application with Impact.*

Repeated impacts on a 3 inch square bar of cast iron over 14 feet span. The general result was that when the blow was sufficient to bend the bar to one half of the deflection at which a static load would break it, no bar was able to stand 4,000 such blows, but if the blow were reduced so that the deflection was only one-third of that required to break it statically then all sound bars resisted 4,000 blows.

2. *Repeated Applications without Impact.*

Other bars were subjected to the action of a revolving cam. Even after 100,000 applications at 4 per minute the bars were not broken if the deflection was one-third the ultimate static deflection. When, however, the deflection was increased to one-half they broke with 900 applications.

3. *Effect of Slowly Moving Weight.*

In another series a weight equal to one-half the breaking load was dragged backwards and forwards from one end to the other of a beam and a sound bar was not apparently weakened by 96,000 transits.

4. *Impact Tests.*

Bars of cast iron were broken by impact and it appeared that bars of the same area offer the same resistance to impact whatever the form of their transverse section. Those tried were $6 \times 1\frac{1}{2}$ inch struck on the broad or narrow side and 3×3 inch bars.

Another set of tests was done in beams with weights attached to them so that they did not prevent the flexure of the bar. These resisted greater impacts from the same body falling on them than unloaded beams in the ratio of 2 to 1.

5. *Effect of Weights Moving with Velocity over a Beam.*

An apparatus was constructed by means of which a car capable of being loaded up to 2 tons could descend from a height of about 40 feet, attain a velocity of 30 miles per hour and run over a pair of cast-iron beams 9 feet long 3 feet apart. Arrangements were made for measuring the deflection and the velocity. The general result obtained was that the deflection produced by a load passing over the bar was greater than that produced by the same load at rest in the middle of the bar. Thus with a carriage of 1,120 lb. at rest in the middle the deflection of a 4 inch broad by $1\frac{1}{2}$ inch deep beam was $\frac{6}{10}$ inch ; with a velocity of 10 miles per hour the deflection increased to $\frac{8}{10}$ inch and at 30 miles per hour it became $1\frac{1}{2}$ inch.

Such results were clearly at variance with the experience on actual bridges ; some engineers even declared that at speed

the deflection was actually less than when the load moved very slowly. It was clear that the relative inertias of load and bridge must have a considerable influence on the deflection. Prof. Willis obtained the differential equation for the path of the load but found the integration of it involved so much difficulty that (to quote his own words) ' I was compelled to request my friend G. G. Stokes, Esq., Fellow of Pembroke College, to undertake the development of it.' Stokes was successful in solving it for two extreme cases, one in which the beam inertia is small compared with the load and the other when the beam inertia is very much greater than the load. The analysis gave an explanation of the experimental results and indicated that in ' real bridges the effects were so greatly diminished as to be of comparatively little importance.'

This result was checked by experiments on the Ewell Bridge on the Croydon and Epsom line. The span was 48 feet and the statical deflection due to the load rather more than one-fifth of an inch. This was slightly but decidedly increased when the engine was made to pass over the bridge and at 50 miles per hour the increase was about one-seventh. Similar results were obtained on the Godstone Bridge. With bridges of greater span the increase would be less.

The final conclusions were :

1. That it appears advisable for engineers, in contracting for castings, to stipulate for iron to bear a certain weight instead of endeavouring to procure a specified mixture.

2. That to calculate the strength of a particular iron for large castings the bars used as a unit should be equal in thickness to the thickest part of the proposed casting.

3. That as it has been shown that to resist the effects of reiterated flexure iron should scarcely be allowed to suffer a deflection equal to one-third of its ultimate deflection, and since the deflection produced by a given load is increased by the effect of percussion, it is advisable that the greatest load in railway bridges should in no case exceed one-sixth of the weight which would break the beam when laid on at rest in the centre.

4. That as it appears that the effect of velocity communicated to a load is to increase the defection it would produce if set at rest upon the bridge, also that the dynamical increase in bridges of less than 40 feet in length is of sufficient importance to demand attention, and may even for lengths of 20 feet become more than one-half the statical deflection at high velocities but can be diminished by increasing the stiffness of the bridge, it is advisable that, for short bridges especially, the increased deflection should be calculated from the greatest load and highest velocity to which the bridge may be liable ; and that a weight which would statically produce the same deflection should, in estimating the strength of the structure, be considered as the greatest load to which the bridge is subject. For the method of calculating this increased deflection, reference is made to Appendix B of the report.

5. Lastly, the power of a beam to resist impact varies with the mass of the beam, the striking body being the same and by increasing the inertia of the beam without adding to its strength, the power to resist impact is within certain limits also increased. Hence it follows that weight is an important consideration in structures exposed to concussions.

The whole report represents an extraordinarily fine achievement for less than two years' work. The preparation alone must have taken considerable time for it is accompanied by a number of plates and the evidence is carefully indexed and summarised both in the subject and under individual names so that it is very easy to find one's way about in it. It contains the first experiments on many subjects of vital importance to engineers and many years were to pass before they were superseded. Except for Fairbairn's famous experiments on wrought-iron built-up girders in 1860-61 nothing of importance was published on the resistance of materials to repeated application of load until Wöhler's classical work of 1870.

Despite the fact that the conclusions did not justify the fears that were entertained by prominent engineers (the first three were merely a statement of the best current practice), it soon became clear that the action of one railway inspector certainly did. On December 24, 1849, he refused approval of the Torksey Bridge built by Fowler on the tubular principle. Certain

errors in his report were pointed out to him but he still withheld approval. The matter caused a considerable stir and was twice brought before the Institution of Civil Engineers. In March 1850 Fairbairn read a paper on tubular bridges, the discussion of which extended over several meetings. It is clear that all were against the inspector's findings and were concerned about the fear of Government boards and commissions being set up which would 'interfere with the progress of mechanical and constructional science.' They feared the introduction of a system analogous to that of the Ingenieurs des Ponts et Chaussées which it was held had proved so detrimental to all individual enterprise in France. The outcome was that though another inspector examined the bridge and agreed with his colleague's decision, on April 6, the commissioners informed the secretary of the Manchester, Sheffield and Lincolnshire Railway that they had reconsidered the question of the propriety of allowing the line to be opened and, having received a further report on the subject of the Torksey Bridge, they were willing to allow it to be used for public traffic on being informed that the recommendations as to ballast had been complied with, and on receiving an assurance from the company that should it appear necessary to the inspecting officer at any future period on an examination of the bridge that it required to be strengthened, the Railway Company would promptly attend to his suggestions. The line was opened on April 28, 1850. Pretty quick work for the settlement of a first-class row with a Government department.

The dominant problem in the design of beams a century ago was the correct proportioning of the tension and compression flanges. The general character of the shear problem was understood, but no satisfactory theory was available until Rankine's *Applied Mechanics* was published in 1858. To those interested in the problems now exciting interest, I would suggest a perusal of the *Colston Papers*. Vol. II, which are based on a Symposium promoted by the Colston Research Society, and the University of Bristol, edited by Prof. A. G. Pugsley and D. R. Rexworthy and published last year.

I began by suggesting that in looking at the history of our subject, we were concerned not only with the actual performance but with the way in which the men responsible for them faced their task. I cannot do better than close with a poet.

'This is the wonder, always, everywhere,
Not that vast mutability which is event,
The pits and pinnacles of change,
But Man's desire and valiance that range
All circumstance and come to port unspent.

Agents are these events, these ecstasies
And tribulations, to prove the purities
Or poor oblivions that are our beings. When
Beauty and peace possess us they are none
But as they touch the beauty and peace of men.

Nor when our days are done
And the last utterance of doom must fall
Is the doom anything
Memorable for its apparelling;
The bearing of man facing it is all.'

THE INFLUENCE OF FOOD PLANTS ON SOCIAL STRUCTURE

Address by

DR. REDCLIFFE N. SALAMAN, F.R.S.

PRESIDENT OF SECTION H

I WILL not delay by apologising for the fact that I, who am neither anthropologist nor archaeologist, find myself in the honourable if embarrassing position of President of Section H. That such has come to pass must be regarded as the responsibility of those who risked putting an outsider in power. I am aware that the impulse for this hazardous step arose from the fact that in my book *The History and Social Influence of the Potato*, I attempted to show that under suitable conditions the structure of a social group may be conditioned by the nature of its staple food. To-day I propose to illustrate this same thesis with specific examples. In the main I shall draw my evidence from the progress of the potato in Great Britain.

We know that the potato arrived in Europe during the second half of the sixteenth century, reaching England about the year 1586, at a time when communications were becoming easier, and printed literature rapidly growing in volume and variety of subject matter ; hence it comes about that in western Europe it never lacked a literary background.

Although it is with the potato that I illustrate my thesis, I believe that as rich, and possibly more satisfying a harvest, awaits the student who may investigate the social repercussions of rice, or of maize, to choose another example from the New World. Doubtless a more difficult task awaits him who undertakes a like research in relation to homegrown cereals, wheat, rye, barley and oats. But so worth while, I am convinced, are such researches, that I look forward to the time in the near future when a greatly enlarged edition of de Candolle's *Origin of Cultivated Plants* will devote most of its attention to the repercussions of the various staple foodstuffs on human social relations.

There is surely a great future for research of this nature which, following Fraser Darling's example, may be described as Human Ecology. It is in this relation that I find it peculiarly opportune and indeed significant that my friend I. H. Burkill is reading a paper in our section on the influence of the Yam on native African peoples.

To-day I am not concerned, except remotely, with the physiological and medical problems arising either from an excess or deficiency of a potato diet, nor need we delay over the gustatory merits of the potato. Not that these subjects are not matters of high importance, but inasmuch as the potato has won the favour of all races and classes in every clime, and that over centuries, we may safely leave these issues out of consideration. A few words as to the value of the potato as a food may not be out of place.

The potato is too often dismissed as being a farinaceous food and little else. This is far from the truth, for although starch accounts for about 80 per cent. of its dry weight, and sugar for another 5 per cent., its protein content is by no means negligible, averaging about 8 per cent., most of which has a high nutritive value. In addition to the presence in useful quantities of a number of the most essential mineral constituents, the potato is a valuable and reliable source of the anti-scurvy vitamin C, and a rather less satisfactory one of some of the members of the B group of vitamins. In fact it needs only the additional ingestion of a daily pint of milk, to make the potato a complete food replete with anti-rachitic vitamin D, and an

increased supply of proteins. Such a simple combination supplies a diet capable of sustaining active and hardworking men and women over an indefinite period.

This is a point of some importance, for had the potato failed as a physiologically satisfactory food, it would have been unable to exert any continuous influence on society. In this relation, compare the fate of the potato with that of the Jerusalem Artichoke, a physiologically unsatisfactory food, which reached France only a decade later. After a short spell of popularity it faded out of the picture without leaving a mark on the structure of society in France or anywhere else.

The only real drawback to a potato diet is its bulk, as may be appreciated from the fact that in Ireland in the old days when an agricultural labourer had little or nothing else to eat, he consumed about 14 lbs. of potatoes per day, and his wife another 7 lbs. It is not likely that we shall revert to these standards, no matter who reigns at the Ministry of Food.

I have not time to make more than a short reference to the history of the potato in its native home of Peru and Bolivia. I should, however, point out that from its earliest contact with man, it exercised a fundamental influence on his way of living.

Several lines of evidence have led me to believe that the first inhabitants to enter the great Andean Plateau, some 14,000 ft. above sea level, reached it from the Amazon basin. But if that much be granted, one is still confronted with the problem of discovering by what means the pioneers were enabled to maintain their foothold in so inhospitable a region. The answer may be summed up in the one word, ' potato.'

The common potato is one of many species belonging to a genus of the Solanum family which grows wild at these altitudes and which from time immemorial has been the basis upon which the domestic life of the natives of these parts has been built. Alternative foods were few and sparse, and of these only two have any importance, the quinoa and occa, neither of which compare favourably with the potato for ease of cultivation or adaptability as a foodstuff available throughout the entire year.

The pre-eminence of the potato was established at an early date, possibly two or more thousand years ago. We do not know when, but certainly prior to 600 A.D., the cultivators of these bleak highland plateaus discovered the means of preserving the potato by alternate freezing and drying, with the result that the final product known as chuño could be kept for an indefinite period, the preserved potato in its capacity of a food reserve gave an added measure of stability to the settlers, whenever, through frost or other cause, the potato crop failed.

Chuño, indistinguishable in appearance and taste from that made to-day, is found in the tombs of the pre-Inca settlements along the Pacific Coast. It is probably man's earliest and most successful discovery in the realm of food preservation. Thus the fresh potato and chuño between them, acted as a life-line to the natives of the altiplano in their unceasing struggle for existence.

Nor was the potato without effect on the structure of native life. In the Indian settlements there are still territorial groups known as ' Ayllus,' kin-groups whose communistic organisation has remained unchanged since Inca times. Much of their field work is still carried out on a co-operative exchange basis known as ' Ayni, whose working may be illustrated by their method of cultivating the potato. Two men dig with an instrument, very like a caschrom, known as a taclla, raising each a clod as they step backwards, whilst two women turn the turves over by hand as they advance step by step towards the men. All four persons are members of the Ayllu and may belong to as many separate households, though the plot itself may be the property of a single individual. Whether the communist principle dictated the type of cultivation or, as is more probable, the local requirements of the crop and its environment suggested the communistic method, I do not know. It is of interest to note that this native Peruvian method of growing the potato for domestic purposes, closely resembles the Irish lazybed system which, till recently, was worked co-operatively on a voluntary basis by 8 to 10 men drawn from neighbouring cabin-holders. Of such vital importance was the potato as a food-stuff

in Pre-Inca Peru that a peculiar type of human sacrifice was practised in order to ensure its fertility. A full account of the available evidence will be found in my recent book.

When the potato reached Europe, polite society was already well supplied with a variety of animal and vegetable foodstuffs. It must not be assumed, however, that the bizarre collection of viands which graced the tables of the great, were known to, much less eaten by, the artisan or peasant of the time. The vast majority of the population continued to look to the cereals, oats, barley, rye and wheat, for their basic food, as they had done for over a thousand years.

Fortunately there is no need to stray beyond the confines of Great Britain to find adequate illustration of my thesis.

If for convenience we take the line of the Trent as dividing the northern from the southern counties of England, it may be said that the potato became an accepted food of the people in the north at least 100 years earlier than in the south. In Lancashire potatoes were being sold in the markets of the larger towns at the end of the seventeenth century and within the next few decades were being grown in the farmer's fields as well as in the gardens of squire and villager. In the south they were practically unknown to the working classes, urban or rural, and were only occasionally grown as a luxury in the gardens of the aristocracy.

This precocious spread of the potato in the north was due neither to the stimulus of poverty nor to the urge born of food shortage. Indeed in the north the working classes, whether engaged in agriculture or mining, were always better paid than their fellows in the south, a consequence of competition between their respective industries. Accustomed to the use of oatmeal and therefore unhampered by the dietetic prejudices of the southerner which centred round the use of wheaten flour, the people of the north found in the potato a useful and pleasant dish which they could readily incorporate into the dietary of the home without loss of caste and without displacing other and earlier established articles of food. The northerners were equally at an advantage when they came to cultivate the new root crop, for in the greater part of the north, the open arable fields, where such had existed, had long since been enclosed, and with that all hindrance to new crops and new methods of tillage had disappeared.

In the midlands and south, except for the county of Kent, and the south-eastern parts of Essex and Suffolk, most of the arable land still lay in the open field, where the rotation of crops and the methods of cultivation followed a century-old traditional course. In fact it was the difficulty experienced by enterprising land owners and farmers in overcoming this obstacle before they could introduce new crops and styles of tillage, which was one of the more important factors which hastened the enclosure and final disappearance of the open field. The potato in particular failed to fit within the framework of the old system. This was due to the fact that in accord with immemorial custom, the open fields, immediately after harvest, were laid open to pasture by the livestock of the stripholders. A potato crop had there been one, would by then have been in full growth and would have inevitably been ruined by the trampling of horse, beast and sheep, which wandered far and wide over the stubbles.

This brief survey may suffice to show that up till the latter half of the eighteenth century the potato had achieved no revolutionary change in the dietary or habits of the English people. In the north it had merely taken its place alongside other staple foodstuffs consumed by the workers, whilst in the south it had met with little or no favour in these circles.

It was not long before the picture was to alter. The social structure of England had in the course of centuries evolved to meet the needs of a dominantly agricultural economy ; within a few decades it was obliged to adjust itself to an industrial form of life based increasingly on the towns. In this reorientation the potato, as a cheap and easily raised food, played an important part.

It is well known that the upper classes, including the larger farmers of the seventeenth and eighteenth centuries, were not only great eaters of meat, but they had also at their disposal an amazing variety of fish and fowl. Vegetables, i.e., greens, onions, radishes, parsnips and turnips,

though always grown by the country folk, had as a product of the market gardens now growing up on the outskirts of the big towns, but recently become available to the town-dweller of the eighteenth century. It was here too that potatoes began to be raised, and that in ever increasing quantities, but it was a long time before the working classes could afford to buy them.

The position in regard to fruit was even less favourable ; although regular shipments of oranges were beginning to arrive from Spain, the price was far beyond the means of an artisan.

In short the worker, urban or rural, was lucky if he had meat on a Sunday and an occasional scrap of bacon or pork on one or two days of the week. In the main he lived on bread, butter, cheese, and milk when he could get it.

With a diet so restricted, it might be thought that the labouring classes of the south would have greedily seized on the potato as a welcome addition to their larder. Not at all ! With few exceptions they held the new food at arm's length, and only with reluctance did they later venture to feed potatoes to their pig and horse. Even so, many expressed doubt whether they would not ' poison the pigs.' The reason for this aloofness may in part have been due to the conservatism common to a peasant society, but in my opinion there were other and more potent forces at work.

During the eighteenth century the Englishman's interest in Irish affairs grew apace. Although he feared the competition of the Irish in trade, and his enmity as a potential ally of France, he never doubted that the mere Irishman was culturally almost a savage, economically little better than a slave, and biologically peculiar in that he lived almost exclusively on the potato. To most townsmen the potato was regarded not only as the emblem of Ireland but the badge of servility, and therefore not a fit food for free men and Englishmen to boot. In the remote countryside where such ideas had failed to penetrate, the opposite view might find expression ; thus a small tenant near Basingstoke, on being advised by the rector's wife to grow potatoes in her garden, replied ' No ! No ! they are all very well for you gentry, but they must be terribly costly to rear.'

For centuries the working classes had suffered from disabilities, political and social, which until recently they had regarded as inevitable and unalterable. But in 1789 it was learnt that men just across the Channel less well off than themselves had risen up and cast off their chains for ever—at least so they thought.

On this side of the Channel whilst it was true that considerable social changes had overtaken the working classes none had made life easier, indeed the contrary was the fact. The wholesale enclosure of commons and open fields, the engrossment of farms with the accompanying destruction of cottages, the introduction of mechanisation in agriculture and, above all, the rapid progress of industrialisation and the consequent drift to the towns, had induced a feeling of unrest and bitterness amongst the working classes but had done nothing to increase their well-being.

Most popular discontents have their roots in a deficiency in food supplies, and those of the eighteenth century were no exception. For centuries in England, various types of bread had been in use which differed one from another in the proportion of wheaten flour used in their composition. In consequence, the colour of the bread, originally the criterion of quality, by inference became that of class. During the latter half of the eighteenth century a subtle change, had overtaken the people's attitude towards their daily bread. Workers in the south who had for countless generations been content with a brown or dirty grey coloured loaf, made of a mixture of wheat with barley or rye now in the second half of the century began to demand a loaf akin to that consumed by their social betters. Before the century was out, employer and worker were consuming the same white wheaten loaf. Any suggestion that they should be content with one less white, was deemed by the workers as a challenge to their self-respect calling for strong resistance.

Towards the end of the century the political and economic atmosphere at home suddenly became tense. The war with France was in full swing and not going well for the allies. The September massacres had wellnigh extinguished what

little sympathy had ever existed between master and man, and had done much to intensify the fear with which political leaders regarded the people.

The harvests of 1792 and 1793 were indifferent, those of 1794 and 1795 disastrous; it was not surprising that as the year closed, deep anxiety came to be felt not only as to the nation's food supplies, but still more as to the temper in which the workers would react to a shortage.

It might be well at this point to call in an independent witness, the Rev. David Davies of Barkham in Berkshire. Writing in 1795, he tells us that the poor had taken to the wheaten loaf because wheat had of late become more abundant, nor had they, as in the past, the quantity of milk at their disposal to wash down the coarse mixed bread.

'To-day,' he writes, 'the whole labouring people have neither meat nor cheese nor beer in sufficient quantities, they eat good bread where everybody else eats it.' He goes on to say that this new fashion had been encouraged by the landed classes because the increased demand for wheat was reflected in increased rents. Many people, Davies observes, complain that the worker neglects the use of the potato both for himself and his pig, to which he comments 'in richer counties the poor have neither the garden to grow it (the potato) nor milk to eat it with. This is due to engrossing, the little scrap of garden left to him he uses for a variety of vegetables, where butter-milk can be got, potatoes are eaten.' Before the print had dried on Davies' page the food crisis of 1795 had overtaken the country, a crisis which was to have a profound effect on the domestic habits and the economic future of the labouring classes.

Alarm, speculation and, it was suspected, manipulation of markets, led to a fantastic inflation of the price of wheat. As a result the price of the quartern loaf rose at once from 7*d.* to 1*s.* 1½*d.* and later reached 1*s.* 7*d.* In the southern and eastern counties wages after much delay increased generally by about 15–20 per cent., but both then and for the next thirty years wages south of the Trent barely kept pace with the growing cost of living.

Efforts were made by those in authority from the Prime Minister and both Houses of Parliament down to the Parish Priest, to persuade the worker to return to a loaf of mixed wheat and barley or, failing that, be satisfied with what was a new and it was hoped, attractive substitute, a loaf in which for every 20 lbs. of wheaten flour, 12 lbs. of raw potatoes were added. A like proposal had been made by Parmentier in 1773 which, the encouragement of Louis XVI notwithstanding, met with no success. The French poor had long since accepted the potato as a vegetable but would have nothing to do with it as a substitute for wheat.

Letters describing new types of bread in which varying proportions of potato were embodied, began to descend on *The Times* like a swarm of flies. Each correspondent boosted his perfect potato loaf, but it might be observed that none proposed to eat it, preferring to endow the poor with the fruits of their ingenuity, whilst modestly retaining for their own household the demoded wheaten bread. A variant on the theme came from no less a person than the Prime Minister. William Pitt, in November 1795, wrote advocating a loaf made of maize and potatoes only, but even his assurance of its excellence failed to make the least impression on the wage earners, who remained strangely unmoved. For them there was to be no going back on the white loaf. For a long time they suffered in silence and only rarely did they, or rather their women-folk, make raids on the food shops, and even then they generally paid what they considered a fair price for the goods they seized.

In the end the white loaf was saved for and by the people, but the victory was exceedingly costly. We can get some idea of what that 'cost' was, by a consideration of the number of loaves which the average agricultural worker could purchase if he were to devote the whole of his daily wage to this purpose.

In the year 1770 he could buy 2 loaves, in 1780 but one and a half, in 1800 only four-fifths of a loaf, and in 1810 a whole one. It was not until 1850 that he could purchase the same amount of bread for a day's wage, as he had been used to do in 1780.

If the worker could not obtain enough of the bread which formerly had been his

mainstay, he had necessarily to look elsewhere for the wherewithal to feed himself and family. It was now, at the time of acute crisis at home and abroad, that the potato, notwithstanding earlier prejudices, obtained its entrance into the homes of the poor, whose dietary during the next half century it to a large extent controlled.

The progress of the potato was rapid. In the Luddite troubles of 1811–12, when shortage of food as much as hatred of the new machines engendered so much bitterness, the demand of the mob was for cheap potatoes no less than a cheap loaf.

The intervention of the potato did, without doubt, ward off actual want, but at a cost which burdened the working classes for generations. Few statesmen or philosophers, of whom Malthus was an honourable exception, realised that salvation at the hands of the potato might be too dear if purchased at the price of the permanent degradation of a nation's standard of life.

During the next fifty years the potato exerted an ever increasing influence on the economy of the workers. They learned to appreciate its merit as a cheap, wholesome and satisfying food of which neither adult nor child ever tired, but they failed to realise that its very cheapness was their undoing. If it had not been that by the middle of the next century the new steam-driven ships had begun to bring wheaten flour and a variety of cheap foodstuffs from America, the masses would surely have been forced to succumb either to a potato controlled economy such as reigned in Ireland, or have substantiated by force their claim to a higher standard of living.

The year 1795 was a critical one, for yet another reason, it saw the introduction of the Speenhamland wage system which did so much to stabilise the pauperisation of England's working classes—a process in which we have seen the potato played the part of a faithful ally. As a result of the new system, rates soared to undreamt-of heights, landlord and industrialist alike realised that in order to check the evil, cheap food and low wages for the working poor were as essential as they were inseparable. But the last thing that either desired was cheap wheat, hence their sudden enthusiasm for the potato, whose heavy crops could be sold at prices which the working classes could afford to pay.

To assure the full use of potatoes by the working poor, the custom of letting small plots of land or allotments at reasonable prices came into being early in the new century. In some cases it was part of the contract that one-quarter of the land should be under potatoes. But such provisions were in fact unnecessary for in most districts it was customary to devote at least half the allotment to potatoes.

Although allotments were originally devised for the benefit of the agricultural labourer, it was not long before the urban artisans demanded the like. By 1815 allotments on the outskirts of the towns were already common and, as in the countryside, they also were devoted to raising potatoes.

In the north, allotments were less common, because largely unnecessary. The workers, rural and urban, had always enjoyed good gardens or, where such were not available, they obtained, often free, ' potato patches ' in the farmer's field.

The development of the allotment system in any district was closely related to the standard of the current wage. The lower the wage, the greater the demand for potato land.

To sum up this portion of my tale—I would say that in the north of England the potato was adopted as a welcome accessory food and, as such, exerted little or no adverse reaction on the social and economic life of the community. In the midlands and the south, where its adoption had been the result of necessity, the consequences were less happy. If the worker by its help, was enabled to cling to his white loaf and at the same time stave off starvation, he was as ever exposed to the full blast of an economy based on the principle that wages, like other commodities, were controlled by the law of supply and demand. As labour was plentiful, wages remained at a level which ultimately was dictated by the cost of subsistence. With the aid of the potato this was kept at a far lower level than would have been possible had it not been adopted as a major source of the workers daily diet.

I now propose to devote such time as remains to me to a consideration of the

same problem in the classical land of the potato.

The association of the words ' potato ' and ' Ireland ' was firmly established long before politicians realised that it might be capable of bearing a sinister interpretation. Indeed the term ' Irish potato ' occurs in the literature as early as 1693 and has been in common use ever since.

Two major aspects of the relationship between Ireland and the potato demand attention. The first is—Why did the potato win such pre-eminence in the domestic life of the Irish? And the second—What has been the effect of this relation on the social history of Great Britain.

The potato was probably introduced to Ireland in the year 1588, reaching the southern counties of Cork and Waterford through the agency of Sir Walter Raleigh ; a separate introduction to Northern Ireland took place in 1606. By the middle of the seventeenth century its culture had spread all over the island, though it was rather more in vogue in the south. Within a further decade or two the potato had become the main and almost the sole source of food of practically the entire Catholic population of Ireland.

When I say ' Catholic ' I do not mean that religion, as such, had anything to do with the people's predilection for the potato. But it was the middle and lower classes, shopkeepers and workers, who came to rely on the potato and they, except in Ulster, were almost to a man native Irish Catholics. The English and Anglo-Irish protestant residents of the official class lived in much the same style as they would have done in England and were therefore no more addicted to the potato than their counterparts at home.

The extent of the potato's domination over Ireland's economy may be gauged by the fact that when in 1846 the crop entirely failed, a disaster ensued unequalled in extent or severity in any other part of the then known world.

When one asks oneself how it came that the potato obtained such a dominant position in the economic life of Ireland, one realises that there is no single answer to the question.

The problem may be approached from several angles—soil, climate and traditional agricultural methods, all played a part. But, however favourable those might be, alone they never would have allowed the potato to attain control over the dietary of the people. We must seek for other factors in the structure of the Irish society which favoured its dominance.

Ireland is blessed in its climate, temperate and moist, and is rarely visited by frost or drought ; indeed with the exception of a relatively small area around Dublin, the rainfall is much above the average enjoyed in England. Such climatic conditions do more than favour the growth of the potato, they constitute a potent protection against the propagation of the more serious virus diseases, to which the potato is prone, and in so doing prevent that degeneration of stocks which is so serious a problem in most parts of England.

The soil, which for the most part is rich, deep and friable, is ideally suited to potato cultivation. The boglands when suitably cultivated, lend themselves better to the cultivation of the potato than to any other farm crop.

Till recent times Ireland was far behind England and Scotland in its practice of agriculture, but nevertheless it possessed an ingenious and successful method of raising crops on undrained land and applied it to the growing of potatoes. I have already alluded to the similarity of this, the Irish lazy-bed system to that still employed by the Peruvian native. The essentials of the method are that the potato seed tubers are laid on the bare turf on a bed some 4 ft. wide. On either side is dug a deep ditch, the contents of which are placed on top of the seed. Nothing further is needed, the furrows, which are about 18 in. deep, act as drains, and are of special value on boggy land, the earth thrown up affords a suitable protection and if the crop is not dug in the autumn, it may be safely left to lie under its cover of clods and be raised little by little as it is wanted. There is evidence that similar methods were in use in early times in many parts of England and Scotland.

A system of land tenure known as ' rundale,' akin to that current in the open fields of England, was common throughout Ireland and was still practised

in the west throughout the eighteenth century. The system, accompanied in many parts by the custom of gavelkind, in practice meant either that a peasant might occupy one very small plot, or that several individuals occupied a rather larger one. In some parts the plots changed hands every year, which had the usual result that no one improved his land. The drawbacks of the system are obvious, but against them was the right of plot-holders to extensive hill pasturage for their cattle. For a peasant population whose subsistence was based on potatoes, the traditional methods of cultivation were admirable.

The lazybeds were prepared in the winter, the potatoes and oats were sown in the spring, after which the men folk were free to wander off to the hills with their cattle, from St. Patrick's Day, March 17, till the end of summer ; there they built booths and were joined by their families. The crops on the lazybed were left untended till they returned.

The pattern of life was simple and in itself led to an easy-going lazy type of existence in which there was neither demand for the refinements of society nor the desire to acquire them.

But we are anticipating : the potato had not yet arrived and when it did do so at the end of the sixteenth century, it found an Ireland ravaged and torn asunder by war. The reigns of Henry VII and Henry VIII had passed without serious trouble as measured by Irish standards, but the latter had sown the seeds of war by insisting on the pretence that the great Irish landlords were feudal lords holding from the throne of England, suppressing the monasteries and debasing the coinage, a policy which was bettered by Elizabeth for, whilst she restored the character of the coinage current in England, she shipped all the worthless coins for circulation in Ireland.

The storm broke in 1566 with the rising of the O'Neil of Ulster. This was duly suppressed when in 1568 the Desmond Rebellion broke out in the south and internecine war continued until 1582. After an interval of thirteen years, the O'Neils once more took the field and behind them the west and south rallied. In the Desmond campaign the harrowing of the country, the destruction of the woods and the cattle, the indiscriminate slaughter of the peasants, men, women and children had, it might have been thought, reached its apogee, but the devastation which accompanied this last campaign was said to eclipse all others. Fighting continued for seven years in the sixth of which a Spanish army of 4,000 men was landed at Kinsale and was defeated by Lord Mountjoy. In 1602 the rebels exhausted, laid down their arms.

It was during these years of turmoil that homeless men, women and children were literally hunted through the wild country sleeping in ditches and feeding on the shamrock. It was a time when no man's life was safe, and none could count on reaping the corn he had sown.

A despatch of Sir Henry Sidney's in 1566 tells its own tale. ' On Clogher in South Tyrone,' he says, ' we stayed to destroy the corn ; we burned the country for twenty-four miles compass and we found by experience that now (the end of September) was the time of year to do the rebel most hurt.'

As the century came to its close, destruction and devastation reached their zenith. Was it surprising that as soon as he could rebuild his home, the Irish peasant should accept with both hands the gift of the potato which, unheralded, had reached Ireland's shores some fifteen years earlier ?

We do not know when the peasants actually began to cultivate the potato, but it could hardly have been later than the first decade of the seventeenth century, at least in the south. The bitter experiences of the Elizabethan wars were still fresh in their minds. They knew only too well that their corn crop, hitherto the mainstay of the family, could be trampled down and destroyed in a few minutes by a raiding English patrol, and that the corn if it had been harvested and put in their huts would share their fate when the soldiery fired them, leaving them at one blow without food or home. With such experiences constantly in their minds it could not have taken the Irish native long to discover the advantages of growing the potato on the lazybed which it should be remembered antedated by many centuries the arrival of the potato.

If the enemy attacked a holding in the spring, the tubers would be hidden by the upturned turves on the bed and no soldiery would ever take the trouble to laboriously uncover them. If the raid took place in the summer time when the potato haulms were in full growth, they might certainly be trampled down, but the bed had only to be left alone and new shoots would grow and the crop, though diminished in volume, would still be safe. If the raid occurred during the winter months, the crop was at its safest for the peasant did not harvest his potatoes in one operation but dug them when he needed them, leaving the remainder snugly hidden under a consolidated covering of turf, safe from raiders and frosts alike.

One cannot say when the peasant began consciously to appreciate the value of these protective tactics, but there is reason to believe that a proclamation of Lord Broghill during Charles I's Irish War issued at Youghal in 1644 refers to their use. In any case by the middle of the seventeenth century the potato had proved its worth in war and peace, and had gone a long way to establishing its monopoly over the dietary of the peasantry.

When one speaks of a monopoly it is to use no idle term. The vast majority of the Irish people during the course of the century following the Cromwellian Wars became increasingly monophagous. In 1653 Vincent Gookin opposing further transplantation, pointed out that the Irishman grows corn only to export it to England, whilst he himself lives on potatoes and the milk of cow and goat. In 1670 Sir William Petty writes 'the food of the people is bread and cakes whereof a penny serves a week for each : potatoes from August till May—as for flesh they seldom eat it,' and again, 'what need they to work who can content themselves with potatoes, whereof the labour of one man can feed forty and with milk whereof one cow will in summer time give meat and drink enough for three men.'

As the lamentable tale of Anglo-Irish history unfolds, the background to the picture remains remarkably constant. On the one hand, a dominant and largely alien minority controls both legislature and executive, owns almost all of the land, is in receipt of most of the liquid income which it withdraws from Ireland and spends in England. On the other, we have a proletariat in the main abjectly poor with a productivity lower, probably, than any corresponding class in Europe.

In one respect these depressed peasants and artisans differed from the poor of other lands by generally having enough to eat or, as Arthur Young used continually to write, 'a belly full of food.' For nine months of the year that food was in the main potatoes, and for the rest, oatmeal and salt.

As the years went by and we enter the eighteenth century, a further stimulus was given to the all pervading influence of the potato by the Penal Laws, whose object was to entice the Irishman to join the fold of the Anglican Church whilst at the same time sapping his political and economic strength by an enforced trade depression. If the first failed, the second was only too successful.

The vertical cleavage of Irish Society into privileged Anglican and depressed Catholic Irish sections became absolute. A few of the latter were landlords, a few wealthy graziers, but the remainder were small struggling farmers, impoverished tradesmen and cottiers who were fortunate if they had the use of an acre of land on which to grow their potatoes. Some might own a cow and most would keep a pig, the sale of which provided the only source of currency the majority of the Irish peasantry knew. The household, that is to say the two parents, their half dozen children, their cow, their pig and their poultry, all lived under the same roof in a hut which often had no chimney and but one small window.

The uniform dominance of a single food—the potato—prepared in every home in the same manner and equally available in the same pot for the family as for the pigs and the poultry, tended to consolidate the native Irish population by reducing the standard of living of the great majority of the people to the lowest that had yet been attained in a Christian land.

Arthur Young, who travelled all over Ireland, has left us with scores of descriptions of the peasant and his living conditions. I will quote one which may be

taken as typical of the highest level of the Irish peasantry : its date is 1777.

Furness, County Leitrim : 'In respect of labour every farmer has as many cottiers as ploughs, whom they pay with a cabin and one acre of potatoes, reckoned at 30*s*. ; and a cow keep through the year, 30*s*. more. Every cabin has one or more cows, a pig and some poultry. Their circumstances just the same as 20 years ago. Their food, potatoes and milk for nine months of the year : the other times wheaten bread and as much butter as the cow gives. They like the potato fare best. Some have herrings ; and others 6*s*. to 10*s*. worth of beef at Xmas. Sell their poultry, but many eat their pig. The sale of the fowls brings a few pounds of flax, for spinning, most of them having some of that employment.'

At the other end of the scale—where indeed the great majority of the peasantry were to be found—we have such descriptions as this of :

Ballynogh, County Cavan. 'The poor live on potatoes and milk, it is their regular diet, very little oat bread being used, and no flesh meat at all except on Easter Sunday and Xmas Day. Their Potatoes last them through the year ; all winter long, only potatoes and salt.'

In a few cases Young found the larger farmers prosperous, but in such the workers were neither better off nor better fed, indeed on the estates of the wealthy graziers where potato land was often scarce and always dear, the reverse was the case.

A dietary based on the potato was so simple and the tubers on the whole so easy to come by, that it tended to extinguish that sense of responsibility which is the hall-mark of good citizenship and the basis of self-respect.

The evil results of such a system were to be seen on all sides and were commented on by visitors throughout the seventeenth and eighteenth centuries. It is instructive to glance at a list of the complaints alleged against the Irish generally. The indictments may be narrowed down to 9 charges :

(1) The young marry at too early an age and without making any provision for the setting up of a home.

(2) Parents are in no way embarrassed by the large size of their families, nor do they take any measures to control the number of their children.

(3) The peasants live like pigs in miserable smoke-besmirched huts and indeed share their dwelling and their meals with both pig and poultry. The cow is a privileged member of the household.

(4) The women and children go about bare foot and often half naked, and almost invariably unwashed.

(5) The menfolk are too lazy to seek work outside their own holdings and do little enough on those.

(6) The men are addicted to drunkenness.

(7) The men, but more particularly the women, spend most of the summer tramping the country, begging.

(8) The peasantry as a whole tend to live on a barter basis and make little use of cash.

(9) The native Irish under-bid the English to such an extent that the clauses in the various plantation schemes by which labourers on the land granted to English planters were themselves to be English have become a dead letter.

Before attempting any comment, one should say that in general all the complaints were well founded ; those concerning the family life, the squalid home, early marriage, large families and the lack of personal hygiene, were not the immediate result of a potato economy for they had all existed to some extent before such came about. What the potato did was to intensify each whilst endowing it with an inviolable stability.

The Catholic population had for centuries avoided all external signs of prosperity. The poorest clothes, the most meagre household equipment, were the rule. There were good reasons for such prudence. The Irish tenant was never secure—his rent could be raised or the land sold over his head at any moment. Poverty was a weapon in his defence mechanism. It was the same with the question of the hated tithe to an alien

Church ; it was a matter of bargaining and the incumbent himself was often soft hearted and yielded to the plea of poverty, though the agents which many of the clergy employed were less likely to be deceived. These characteristics were, however, stabilised by and maintained under the potato system which made life relatively safe so long as it was lived on the lowest economic level.

Laziness is surely one of the original sins we all share, if only circumstances allow us the opportunity for its indulgence. To the peasant who could feed his family and his stock on the proceeds of a few months work each year in the potato field, laziness became an inevitable characteristic.

Drunkenness presents an interesting problem. During the seventeenth and most of the eighteenth centuries, the peasantry were by no means addicted to excessive drink, a vice which at that time was rather a characteristic of the upper classes. But later a change came over the scene, as a reaction to the growth of unemployment and general frustration of the working classes produced by the Penal Laws. A further stimulus to spirit drinking was occasioned by the Act of 1758 when the distillation of spirits was prohibited. We have seen in our own time that this type of legislation invariably carries with it its own negation ; it did no less in Ireland. Illicit stills sprang up on all sides, especially in the wilder parts of the countryside, where, secreted in the bogs, they escaped detection. The basis of the ' wash ' to be distilled was potatoes, the finished product or whisky is known as ' poteen.' Before long its consumption became firmly established in the homes of the peasants and has remained there, so far as I know, till to-day.

Begging on a mass scale became a firmly established custom during the eighteenth and first half of the nineteenth century and in this form may be regarded as a by-product of the potato economy. Two major factors played a part : one that the men folk left the homes after they had sown the potatoes, to go creaghting with the cattle on the hills, leaving the womenfolk and children at a loose end at home. The other and doubtless more compelling factor was that by the end of spring, the stock of potatoes had given out and would not be renewed at the earliest till August. Alex Thom described the custom as it worked in 1728, as follows :

> ' Native Irish in the mountains, that have homes and small farms where they have sown their corn and planted their potatoes, and cut the turf for firing, do either turn out their cows or send them to the mountains, then shut up their doors and go abegging . . . Thus they pick up enough to pay for their rent, and by the help of their cattle, corn and potatoes, live idle the whole winter.'

It is only fair to add that begging on the scale it attained in Ireland is neither a measure of depravity nor unimpeachable evidence of penury. It may be regarded as an extension of an age-old trait linked to the custom of creaghting during the summer months. The new direction it took on in the eighteenth century was the result of the economic helplessness induced by the political structure of Irish life and the economic bondage of its people to the potato which left unbridged the gap between the end of the old and the harvesting of the new crop.

So completely did the potato control the standard of living of the Irish peasant that most of his transactions were carried on by barter, not of goods, but of labour in exchange for goods, or, more specifically in lieu of rent for the annual use of his potato land. In fact the tragically low standard of living which had resulted from generations of a potato-ridden economy, occasioned a specially designed form of contract which established itself throughout Ireland, known as ' conacre.' The peasant entered into an agreement with a neighbouring farmer for a year less a day, at a certain price depending on which party cultivated and manured the land, the rent was paid, not in cash but by the labour of the tenant, valued at a rate determined by bargaining which was invariably below the standard for piece work.

It would be easy to cite a longer array of facts to show how the potato came to dominate the life of the Irish people. I will content myself with the last and crowning argument.

The potato crop failed completely in 1845 and 1846. There ensued a disastrous famine because the people—not the country—had (by and large) no other food reserves and knew of no other agricultural activity beyond the raising of potatoes. As a result, two million people either died of want or left the country within the next four years. Those who emigrated to the United States of America sowed the seeds of Anglophobia which, after one hundred years, is still alive and often active.

I may now sum up the burden of my address as follows: An easily grown, cheaply produced, substantially efficient and pleasantly tasting food can, under certain political and economic conditions together or alone, fatally menace the social well-being of the people who adopt it.

In England, in the late eighteenth and early nineteenth centuries the lower wage level was controlled by the supply of labour and the higher by the Speenhamland convention. The cost of bread, meat and milk on the other hand was the subject of supply and demand and fluctuated often wildly. Under such conditions the adoption by the workers of a cheap food like the potato, automatically prevented a rise in wages. The price of bread might be almost prohibitive, and that of meat beyond the labourer's means, but it could not be said that the people were starving and incapable of work, so long as they had the potato to fall back on. The effect therefore of the potato was to stabilise the standard of life at a lower level than would have prevailed had no such substitute food been available.

In Ireland, the forces which gave the potato its unexampled dominion over the life of the people were political, economic, racial and religious—no mean combination. The political and economic degradation of the Irish people had been intentional, and the use of the potato had made it absolute. So long as the potato was economically the least common denominator of the art of living, it was easy to hold a nation in subjection. But the potato economy did more than depress the standard of living to an irreducible minimum, it went a long way towards shaping the way of life of a whole people. Its influence, both in its industrial and in its communal aspects, can be traced in the structure of even nineteenth century Irish Society.

This unique despotism outlived the political duress which had nurtured it. Not even the complete loss of the crop by blight, and the subsequent famine, were sufficient to open the eyes of the people either in Ireland or England, to the danger of building the economic life of a country on so narrow and treacherous a basis.

If I have stressed the case of Ireland as showing the danger of the dominance of a cheap good food, it is because it is so excellent an example of my thesis.

Another example might be drawn upon to illustrate the same theme, but in which both political and religious factors—so imminent in Ireland—were absent: I refer to the influence of the potato in the Highlands of Scotland after 1745. But this is another story of which neither the time at my disposal nor your long-tried patience will sanction the telling.

HORMONES OF THE DIGESTIVE TRACT

Address by

PROF. R. J. BROCKLEHURST

PRESIDENT OF SECTION I

THE automatic regulation of function according to the needs of the body is one of the most engrossing objects of study for the physiologist. Co-ordination of this kind is carried out mainly by the nervous system and by the system of hormones. The former, by reflexes of various degrees of complexity, most frequently acts independently. Hormonal adjustments also are sometimes independent mechanisms, though we often find them associated with nervous processes.

Hormones secreted by the endocrine group of glands, with certain exceptions, have a relatively diffuse or widespread influence in the body. The hormones which form the subject of this address, however, do not belong to this group but to an independent system of their own, and their actions are strictly localised. The digestive tract is a long one and is dealing with the average meal for a considerable time. The desirability for co-ordination in such a system is obvious, and we know that a considerable part in the regulation of its movements and of the secretion of some of its glands is played by the nervous system. But it possesses in addition a unique collection of intrinsic humoral mechanisms which serve to regulate glandular secretion and, to a small extent, muscular activity to the processes of digestion.

Perhaps the most striking feature of these mechanisms is that they are independent of the nervous system, and there is nothing which corresponds to a co-ordinating centre for them ; each mechanism is brought into play, when it is needed, by a particular event in the sequence of digestive processes. As far as we know at present, they are found only in connection with the stomach, the small intestine and its associated glands.

Physiological stimulation of salivation is essentially a nervous phenomenon and takes place immediately the appropriate stimuli occur. The capacity of reflex actions to occur rapidly is undoubtedly more suited to salivary gland excitation, owing to the short stay of food in the mouth, than would be a chemical form of stimulation—by its nature a procedure with a much longer latency. In the stomach and small intestine, however, there is not the same need for immediate action. Although, in the case of the former organ, nervous excitation is the first form of stimulus normally received by its glands this is followed by a chemical excitation ; and a similar form of stimulation is very important in connection with the flow of pancreatic juice, bile and the succus entericus.

Another important characteristic of the digestive tract hormones is their chemical nature ; as far as has been determined, they are all proteins or closely related substances. Hence exposure to proteolytic enzymes inactivates them ; and the only reasonably secure route they can take from their source to their destination is by the blood stream, though even there some of them find specific enzymes awaiting them and cannot long remain active. This, however, is not a disadvantage since the natural agencies producing their discharge are long-lasting and the hormonal action is therefore maintained for just as long as it is needed.

The discovery of the first of these hormones, secretin, was an eventful one, not only in connection with the functions of the digestive system, but for Physiology as a whole. It was the first clear demonstration of the liberation of a chemical substance from an organ into the blood stream and its specific action on another

organ. From this discovery, in fact, sprang much of the work on hormones of all kinds which has gone on during the first half of the present century and has become such an important branch of our science.

Stomach

Some of Beaumont's well-known observations (1833) suggested that some form of chemical stimulation of secretion may occur in the stomach ; but the first important systematic experimental studies of gastric secretion were undertaken by Heidenhain and later by Pavlov. As is well known, the reflex (now called cephalic) phase of gastric secretion was thoroughly investigated by the latter, the Pavlov (or innervated) pouch being the important development in this study, a great improvement on the denervated pouch of Heidenhain. It is now considered that Pavlov's conception of the routes taken by the gastric branches of the vagus nerves was not entirely correct, and improved pouches have since been devised, which are recognised to provide more fully innervated preparations than the Pavlov type.

Although Pavlov was led to attach great importance to the reflex excitation of the gastric glands resulting from the stimulation of taste receptors and, as a result of conditioning, of other sensory endings, plenty of evidence accumulated which showed that in animals with gastric fistulæ secretion could be produced by contact of food with the gastric mucosa. The natural assumption at first was that the food stimulated receptors in the stomach wall, resulting in a true reflex secretion ; but when it was found that secretion occurred even in an extrinsically denervated stomach, the Russian school postulated the existence of local reflex arcs in the stomach wall as the essential mechanism for this process. It is interesting that the importance of nervous mechanisms had become so impressive that, in the absence of evidence of any other obvious possibility, it seemed reasonable then to conclude that a local reflex process must exist.

The discovery by Bayliss and Starling of the secretin mechanism of pancreatic stimulation (1902) opened up a large field of investigation in the digestive system and not least in the stomach. Edkins (1906) was led by it to investigate the possibility of a hormonal form of gastric stimulation and discovered that extracts prepared from the mucous membrane of the pyloric part of the stomach and injected intravenously brought about gastric secretion, this giving rise to the concept of 'gastric secretin' or Gastrin. Since his work there has been developed (Ivy and Farrell, 1925) the important technique of autotransplantation of gastric pouches. A portion of the stomach is brought with its blood vessels under the skin of the mammary region and transplanted in the new site, from which it eventually obtains a local blood supply. The original blood and nerve supply is then cut off, so that a denervated pouch is obtained whose secretion can be collected. Any stimulation of the glands of such a pouch when, for example, food is put into the stomach itself, must occur as a result of a humoral mechanism, i.e. through the blood stream, and not by a nervous process. Such a preparation does in fact respond by secretion to the stimulus of food in the main stomach.

Two other techniques have been evolved with the same purpose of determining whether a humoral mechanism exists. One consists of cross-circulating two animals and observing the response of the gastric glands of one of them when substances are put into the stomach of the other ; the second (Lim and Necheles) involves the procedure known as vividialysis, i.e. dialysis of the circulating blood of an animal and investigation of the dialysate for the possible existence therein of a gastric secretory excitant.

The results of numerous experiments on these lines have made it abundantly clear that the presence of food in the stomach is followed by the discharge of some substance into the blood stream, which, thus endowed, becomes excitatory to the gastric glands. The intravenous injection of some secretagogues which are known to stimulate gastric secretion from within the stomach often results in secretion, so there is no proof here that the physiological excitant is of a hormonal nature. Parenteral injection of extracts of a variety of tissues also has long been known to elicit gastric secretion of an acid type, accompanied by a fall in arterial blood pressure,

the depressor effect being approximately proportional to the stimulating effect on the gastric glands. Investigations showed that the depressor substance in these extracts, and the substance which evoked acid secretion, appeared to be histamine. Both activities are destroyed by the action of histaminase, and there is no doubt that histamine is the agent which produces both effects. But it has been shown that extracts of certain tissues, such as liver, from which histamine has been removed, are much more effective as gastric stimulants when placed in the stomach than when intravenously infused ; and there is no evidence that such substances while in the stomach undergo a change which makes them substantially more powerful as excitants. The probability, therefore, is that a hormone is concerned.

The early work on gastrin appeared to demonstrate that this substance is liberated by partly digested protein food from the mucosa of the pyloric portion and exerts its effect on the parietal cells of the glands in the body of the stomach, the secretion being essentially acid with only a low concentration of pepsin. The gastric phase of gastric secretion has been investigated (Gregory and Ivy, 1941–2) by transplanting a pouch from the body of the stomach into the mammary region and denervating it, denervating and making the rest of the stomach into an isolated pouch, and anastomosing the œsophagus to the duodenum. Secretagogues such as liver extract, when placed in the main stomach pouch, were found to evoke secretion in that pouch and in the transplanted pouch, unless procaine has been previously applied to the mucous membrane of the main pouch. It is difficult to believe that local anæsthetisation of the stomach mucosa could alter secretagogues in such a way that they would be ineffective after absorption ; and this experiment therefore strengthens the possibility that a hormone mechanism exists. The action of such a hormone is not itself antagonised by local anæsthetisation of the mucosa ; if, before placing secretagogues in the main pouch, the mucosa of the transplant was treated with procaine, there was no hindrance to secretion by either pouch. Apparently, then, the procaine prevents the release of gastrin from the mucosa without disturbing its action on the stomach glands. Removal of the pyloric portion from the main pouch did not eliminate the response of the rest of this pouch to intragastric stimuli, which makes it appear that gastrin may be produced by the body of the stomach as well as by the pyloric region. Various workers have found that excision of the pyloric region results in a depression of the acid secretion by the body of the stomach ; though it has been noted on a number of occasions that after the pyloric part has been removed there is a tendency for the secretion to return slowly to normal.

Any doubt that might arise from the possible absorption of secretagogues has been overcome by the discovery, in an experiment in which transplanted pouches of both parts of the stomach had been made, and therefore complete severance of any possible nervous connection between the two, that distention of a pyloric pouch by a balloon can evoke secretion from the body of the stomach.

There is still the possibility that histamine may play a part in the humoral stimulation of gastric secretion. It is known to be the most potent stimulant of the acid-secreting cells of the stomach, and it is found in relatively large amounts in the mucosa. The minimal effective dose, given intravenously, which evokes acid secretion in the human stomach is of the order of 0·004 μg./Kg./min. If this dose is slightly increased there is cutaneous flushing and a tendency to headache. It might therefore be expected that if the gastric humoral stimulant is actually or mainly histamine these symptoms would occur when a meal of average size is eaten. As such is not the regular result of taking a meal, it is at least very doubtful if appreciable quantities of histamine are liberated into the blood during gastric digestion. Some observers have found, however, that in order to produce a detectable rise in blood histamine it is necessary to inject about 1,000 times the minimal dose which stimulates the secretion of gastric juice.

There appears to be no doubt that a gastric secretory excitant which is histamine-free is extractable from the pyloric mucosa of various animals. One of the most effective preparations of this substance, obtained by Komarov (1938–42),

only stimulates the secretion of an acid gastric juice. It is active in animals whether anæsthetised or not, but must be administered intravenously to be effective. It seems to have all the characteristics of gastrin. It is destroyed by proteolytic enzymes ; but it has not been found in the mucosa of the body of the stomach. A similar substance, of a protein-like nature, has been obtained in a considerable degree of purity by Uvnäs, who finds that in many chemical characteristics it is very similar to secretin and that it is active in doses of the same order. It appears to stimulate only the oxyntic cells. Gastrin has been found in the pyloric portion of the human stomach.

The evidence therefore is that gastrin is unlikely to be histamine. Histamine may, nevertheless, be of physiological interest in connection with parietal cell stimulation. While its concentration in the portal vein blood apparently does not rise during gastric digestion, it has been found to be present in gastric juice in at least as high a concentration as in the blood, and one must conclude either that the gland cells may actively concentrate it in passing it into the juice or that histamine arises actually from the mucosa itself. It is present in the juice secreted from the body of the stomach, whether the stimulus has been primarily nervous or primarily chemical. In fact, Babkin suggested that histamine may be the final local stimulant of the oxyntic cell, being liberated in the mucosa by the extrinsic stimulus and passing directly into the secreted juice, and support for this hypothesis has been increasing.

It is now generally agreed that gastrin stimulates the production of a strongly acid juice ; some investigators, as a result of experiments in which the glands of the body of the stomach were stimulated only by humoral means, consider that it may not be responsible for enzyme secretion at all.

Within recent years some experiments (by Uvnäs) have appeared to indicate that even the effect of gastric vagus stimulation depends to some extent on a humoral agent derived from the pyloric portion of the stomach. If this part is removed or its blood supply obstructed, the vagal juice is greatly reduced in volume and acidity. Application of cocaine to the pyloric mucosa produces a similar effect, whereas an injection of cocaine does not inhibit secretion by the glands of the body of the stomach when they are stimulated by histamine. The intravenous injection of pyloric mucosal extracts increases the response to vagal stimulation.

An interesting type of cross-circulation experiment indicates clearly the close connection between vagal stimulation and a humoral mechanism. The blood flow from a cat's stomach was passed continuously into the circulation of another cat, whose pyloric mucosa had been either removed or treated with cocaine. Prolonged vagal stimulation in the recipient cat resulted in a scanty non-acid secretion from its stomach. Stimulation of the vagi in the donor animal, however, resulted in a considerable acid secretion by the recipient's stomach ; so it appears that the presence of the normal pyloric region may be necessary for the development of the humoral process associated with stimulation of the vagus.

Experiments with certain drugs have thrown an interesting light on the possible connection between the nervous and humoral types of excitation. Atropine has been found to prevent the secretory response to mechanical distension or to secretagogues acting from within the stomach, though it does not abolish the response to the injection of gastrin extracts. Further, it has been shown that the application of acetylcholine to the pyloric mucosa causes a considerable release of a chemical secretory excitant into the blood. There is obviously the possibility here that gastrin may be released by a local cholinergic nervous mechanism, the receptor part of which can be paralysed by local anæsthesia and the effector part by atropine.

On the other hand, sham-feeding an animal with a Heidenhain (denervated) pouch evokes secretion in the main stomach, but not in the pouch. Were gastrin liberated as a result of the reflex stimulation it would be expected to stimulate the pouch also ; so it cannot be regarded as proved that vagal stimulation results in the discharge of gastrin.

In researches on gastric secretion more attention has been paid to the secretion

of acid than to the secretion of pepsin. It has long been known that stimulation of the vagus results in the formation of a juice rich in pepsin. Pavlov showed that the enzyme content of the juice depends on the nature of the food which enters the stomach ; and others in his laboratory and elsewhere have demonstrated that mechanical stimulation of an isolated pyloric pouch evokes enzyme secretion by the stomach body. These facts make it probable that a chemical mechanism is concerned in pepsin secretion. Extracts have in fact recently been prepared from the pyloric mucosa which raise the pepsin output of the gastric glands when injected intravenously (Uvnäs). As there was no quantitative relationship between the pepsin and the acid secreted, it seems likely that the active substance is not identical with gastrin.

Less is known about the third, or intestinal, phase of gastric secretion. Although it was demonstrated some years ago that food in the duodenum results in secretion by the stomach and by isolated stomach pouches, and that therefore a humoral mechanism is probably concerned, the actual nature of the stimulus has not been made clear. The discovery of gastrin in the duodenum of man and pig makes it possible that this substance may be the actual humoral agent in this phase of gastric secretion. It is interesting that the presence of acid in the small intestine stimulates the output of pepsin from the stomach.

While it is clear that gastric secretion can be stimulated by the presence of food in the stomach and in the upper small intestine, it has been known since the work of Ewald and Boas (1886) that fat may act as an inhibitory agent. These investigators found that when olive oil was added to a test meal, gastric acidity was lowered and the emptying time of the stomach was delayed. This finding was followed up in Pavlov's laboratory where it was discovered that fat produced the inhibitory effect, not when it was in the stomach, but only after it had passed into the duodenum. A reflex was presumed.

The autotransplanted pouch technique was later applied to this problem, and it became quite clear that a pouch of this sort taken from the body of the stomach suffers an inhibition of its acid secretion and of its motility when a fat meal is administered ; reflex action is therefore not a necessary part of the mechanism. As would be expected in investigations of this type, the next step was to inject intravenously chyle and fat emulsions ; but this procedure had no effect on the pouch, and it was therefore concluded that fats do not produce their inhibitory effect on the stomach after absorption into the circulation but presumably by the liberation of some specific substance. Bile is not concerned in this inhibition.

Tissue extracts have been tested for possible inhibitory action on gastric secretion by injecting them after secretion has been started and maintained by continuous stimulation. Certain extracts made from the mucosa of the upper part of the small intestine are effective, and the existence of a chalone which reduces gastric secretion therefore seems certain. The name which was given to this substance by Lim is Enterogastrone. It has been found that care must be taken in testing for its presence, as nausea and various other disturbances will also inhibit gastric secretion ; and pyrogenic substances in the mucosal extracts have a similar effect. However, enterogastrone preparations have been obtained free from pyrogens, vasodilator substances and other known intestinal hormones. To be effective they must be administered parenterally.

The secreto-inhibitory action of enterogastrone appears to be mainly, if not entirely, on the acid secretion. This has been shown in man as well as in other mammals. The volume of juice is not markedly inhibited, and the secretion of mucin and of neutral chloride is unaffected.

A fall in pepsin secretion has been noticed when fat is present in the small intestine ; but as this effect does not occur when the vagi have been cut, it is not by any means certain that it is mediated by enterogastrone. A careful analysis of this phenomenon is needed.

While it appears that fat is the only substance so far proved to release a hormone inhibitory to gastric secretion, the inhibitory effect on gastric motility has been shown to occur when other substances are present in the small intestine. Soaps and fatty acids are more effective in this

respect than neutral fats, and sugars also produce a similar result; mineral oil seems to be quite ineffective. These effects do not depend on the presence of food in the stomach; fats in the duodenum have been shown to inhibit hunger contractions in the human stomach and in a transplanted pouch in the dog.

The inhibitory effect of fats and sugars on gastric motility has been found by some investigators to be reduced if vagotomy has been previously carried out. Their action may thus be partly dependent on what has been called the enterogastric reflex. Others, on the other hand, have shown that the delay in emptying of the dog's stomach produced by adding oil to a fluid test meal is as great after cutting the vagi as before.

There seems to be no doubt of the existence of an enterogastric reflex by means of which certain substances, including acid and sugars, in the upper small intestine inhibit stomach motility, the gastric vagus being an essential part of the arc. While fat in the intestine inhibits motility alike in the vagally innervated or denervated stomach, it appears so far that enterogastrone extracts are only effective in this respect on the innervated stomach. It is clear that more work is needed on the action of the hormone in this connection. Attempts to separate the secreto-inhibitory component from that inducing muscular inhibition have been successful, and we should now, perhaps, confine the name enterogastrone to the substance which has only one of these actions.

The greater the concentration of the motor inhibitory factor the shorter the latency and the longer the duration of the inhibitory effect. If the intestinal stimulus is powerful enough it reduces not only the rhythmical contractions of the stomach but also the basal tone of the muscularis. Observations of delayed emptying of the stomach produced by substances in the intestine, which might have been interpreted as due to high tone of the pyloric sphincter, have been supplemented by experiments in which the intragastric pressure has been recorded from a balloon in the stomach; and it now seems that, since the pyloric sphincter tone is also inhibited, the delay in emptying is due essentially to a general lack of expulsive force of the stomach musculature rather than to an obstructive effect.

A substance which inhibits gastric secretion and, to a less extent, gastric motility was obtained from urine in 1940 by Gray and others and was called by them Urogastrone. This substance might well be considered outside the scope of this address, unless it could be proved to be an excretory form of enterogastrone or of some other alimentary hormone. There is apparently no relationship between the presence of fat in the intestine and the excretion of urogastrone; and experimental removal of the small intestine lowers but does not abolish its excretion; so there is as yet no certain proof that it is directly connected with enterogastrone. It merits some attention, however, on account of the nature of its action.

Urogastrone disappears from the urine after hypophysectomy, and this has been linked with the fact that there is frequently an improvement in patients with peptic ulcer during pregnancy. Sandweiss discovered that extracts of pregnancy urine prevent the formation of ulcers in dogs on which the Mann-Williamson operation has been performed, i.e. the formation of a gastrojejunostomy and the anastomosis of the duodenum to the lower ileum (the bile and pancreatic ducts therefore open into a part of the small intestine remote from the upper jejunum). Ulcer formation in the jejunum of animals so prepared is almost invariable as a result of the acid gastric secretion coming into direct contact with the intestinal mucosa unprotected by the neutralising properties of the pancreatic juice and the bile, and death from this cause occurs in most of these animals in two or three months. Because chorionic gonadotrophin has inhibitory properties similar to those of urogastrone, and since urogastrone excretion is more profoundly affected by removal of the pituitary than by removal of the ovaries, it has been suggested that urogastrone may be a derivative of pituitary gonadotrophin.

While its excretion is noticeably increased in pregnancy, it is also found in the urine of non-pregnant females and in that of males. The interesting fact about its action is that animals are protected from ulcer formation for about a year after treatment has been discontinued. Human

patients liable to peptic ulcers have undergone prolonged treatment with this substance and have benefited by a reduced tendency to ulcer formation, though results are not always satisfactory when enterogastrone is used.

A careful analysis of the urine and of extracts of the small intestine mucosa has shown that there are present apparently three distinct substances which have different actions : one which inhibits acid gastric secretion, one which inhibits gastric motility, and one which has an intestinal mucosa-protecting action. As the latter does not owe its properties to the reduction of gastric acidity Sandweiss (1943) has suggested a distinctive name for it, Anthelone. Extracts containing it have had beneficial effects in chronic ulcerative colitis. It is at present impossible to say that this substance fulfils the strict requirements necessary for it to be regarded as a hormone ; and a good deal of further clarification of these inhibitory substances and of the details of their actions is needed.

Pancreas

Claude Bernard (1872) noted the secretion of juice from a pancreatic fistula after feeding the animal. Towards the end of the last century investigators in Pavlov's laboratory found that hydrochloric acid in the intestine was a powerful excitant of pancreatic secretion. Its effectiveness was greatest when it was allowed to act in the duodenum and gradually fell off when the acid was placed further and further down the small intestine ; there was no stimulation from acid in the stomach or in the large intestine. As the effect was proved not to be produced by absorption of the acid into the blood, and as it still occurred after section of the extrinsic nerves, Popielski concluded that it must be mediated by a local type of reflex arc through the pancreatic ganglia ; he was inclined to regard mechanical effects in the stomach and small intestine as the essential stimulus for this ' reflex.'

In the course of their investigation of local intestinal reflexes, Bayliss and Starling (1902) proved beyond all doubt that a humoral mechanism was involved, since pancreatic secretion resulted from the presence of acid in a denervated loop of intestine. Their next step was to scrape off the duodenal mucous membrane and grind it up with sand and hydrochloric acid. After removal of proteins by boiling, neutralising and filtering the fluid, the extract was injected intravenously into an animal, when, as Bayliss subsequently wrote, they ' were naturally delighted to find that a copious flow of pancreatic juice was the result.' The name Secretin was given to the active substance thus extracted.

This discovery was confirmed by many other observers, and it became apparent that as the secretagogue activity of unheated mucosal extracts rapidly disappeared without simultaneous loss of vasodilator activity the secretory stimulant effect was independent of vasodilator substances, pointing to a specific secretory hormone ; there is now absolutely no doubt that this hormone is quite free of vasodilator properties and by itself does not affect the blood flow through the gland.

Clear-cut proof of the existence of the hormone has been given by cross-circulation experiments in which acid in the duodenum of one animal evokes secretion not only by its own pancreas but also by that of the animal which receives its blood. This has been substantiated by the demonstration of secretion by an autotransplant of the pancreas when chyme leaves the stomach, or when acid is placed in the duodenum or in a transplanted jejunal loop, and by pancreatic heterotransplants.

Pancreatic secretory activity rapidly subsides as the upper part of the small intestine empties after a meal. This is readily explained by the fact that extracts of the mucous membrane contain most secretin when obtained from the duodenum and progressively less in descending the small intestine. This distribution is not due to the effect of food in the intestine, as it has been found in fœtuses and in infants which have died before taking any food.

Bayliss and Starling, finding that neutral aqueous solutions and alcohol did not extract active secretin from the intestine, believed that the hormone must exist in the mucosa in the form of a precursor, prosecretin, which required activation by acid. But others since have found that active extracts can be prepared with bile

salt and other solutions without the use of acid; and intestinal digestion is apparently normal in cases of achlorhydria and after gastrectomy. Even the expression of juice from the small intestine without the use of any extracting agent is sufficient to remove some secretin from the mucosa. It is therefore unnecessary to postulate the existence of a secretin-precursor in the mucosal cells. Nevertheless, although the presence of protein, fat and carbohydrate in the duodenum can evoke secretion from a transplanted pancreas, without their own products of digestion in the blood being effective, the contact of acid with the mucosa is probably the most potent agent in causing the discharge of secretin into the blood. A *p*H of between 4·0 and 5·0 in the duodenum is sufficient to evoke pancreatic secretion; but there is no exact relationship between the degree of duodenal acidity and the pancreatic response.

The intravenous injection of an adequate dose of an active preparation of secretin usually results in the commencement of a copious pancreatic flow within a half to one minute. The flow of juice quickly reaches a maximum and then slowly subsides. Even after a large dose the stimulating effect always ceases within half an hour. The substance must be remarkably potent; it has been estimated from experiments in which a known dose was quickly injected that one part of secretin in several million parts of blood is sufficient to stimulate the pancreas. Secretin activity is not found in urine extracts, and it has been shown that the hormone becomes inactivated in the blood under conditions which suggest the presence there of an enzyme, secretinase. This destruction is to some extent inhibited *in vitro* by the presence of vitamin K; and observations on animals which have been treated with this vitamin show that the potency of injected secretin is probably enhanced by this procedure.

The ineffectiveness of secretin administration by mouth has been assumed, from its protein nature, to be due to its destruction by proteolytic enzymes; but as it now appears that it is not destroyed by crystalline pepsin, trypsin or chymotrypsin, it is probable that secretinase is present in the stomach or upper intestine.

Continuous infusion of secretin, or a succession of injections, can maintain quite a copious pancreatic secretion for hours without any fatigue being apparent in the gland and without any obvious histological changes in its cells. Since secretion occurs in a denervated pancreatic transplant, it seems likely that secretin acts directly on the acinar cells. It is, however, interesting that atropine may slightly reduce the response which normally occurs when peptone is placed in the duodenum; but this atropine effect is almost negligible when acid is used to evoke the mechanism. It has been suggested that the unstimulated post-ganglionic fibres of the vagus may constantly liberate very small quantities of acetylcholine, which might potentiate the secretin effect; but there is inadequate evidence for this.

Quantitative studies of the action of injected secretin have shown that within the range from the minimal to the maximal stimulus the pancreatic response is proportional to the dose. This has been determined in man with the use of a duodenal tube to collect the secretion, as well as in animals with cannulation of the pancreatic duct. The technique in human beings has in fact been applied as a test for pancreatic efficiency, whose use is increasing. In pancreatic injury or disease the response to secretin injections is diminished; there is first a reduction in the volume and bicarbonate content of the juice; later the enzyme content falls. It has been found possible in this way to distinguish between disturbances of fat digestion due to pancreatic insufficiency and conditions in which fat absorption is at fault.

The detailed nature of the action of secretin came under scrutiny when it was noticed that the enzyme content of the pancreatic juice varied with the method of preparation of the secretin extracts. In 1925 Mellanby found that repeated intravenous injections of secretin, while promoting a continuous flow of pancreatic juice of constant $NaHCO_3$ content (0·14 N), failed to maintain the initial concentration of enzymes, which in fact progressively fell during the experiment. However, when the enzyme output had fallen to a low level it could be restored by

the injection of the vagomimetic pilocarpine, indicating that prolonged stimulation by secretin did not fatigue the enzyme-secreting mechanism. Earlier histological studies by the Russian school had shown that, while vagal stimulation resulted in a loss of zymogen granules from the interior of the acinar cells, stimulation by secretin left these granules apparently unaltered, but produced the appearance of channels of water flowing through the cells.

So arose the conception of a qualitative distinction between the juice secreted by vagal stimulation and that evoked by the action of secretin; the former being essentially a digestive juice, the latter a watery alkaline secretion which serves to create a satisfactory environment for the pancreatic enzymes by neutralising acid in the gastric chyme entering the small intestine. While the earlier work on this aspect of the subject was carried out with very impure preparations of secretin, it has become clearly established that highly purified preparations have the action of stimulating the secretion of a watery alkaline juice, containing only a small concentration of enzymes. After the enzymes in the gland acini have been washed out by the first drops of the secretin-juice their concentration falls to a basal level which seems to be unaffected by the rate of water secretion; and it is now believed that secretin may have little or no effect on enzyme discharge.

The acceptance of this hypothesis as a complete explanation of physiological pancreatic stimulation is, of course, difficult to reconcile with the fact that pancreatic juice during digestion contains a substantial amount of enzymes at a time when reflex stimulation from the upper end of the alimentary tract has ceased, unless there be a reflex acting from the intestine itself which results in continuous stimulation of the pancreatic vagus while chyme is present in the duodenum and upper jejunum. It has been shown that distention of the duodenum, at least, does not reflexly stimulate the pancreas. But the administration of a meal to an animal whose small intestine has been entirely deprived of its extrinsic nerve supply results in an increase in the enzyme output of the gland; and the entry of food into the duodenum can produce a similar effect on a denervated autotransplanted pancreas. Such stimulation of enzyme secretion therefore results from a humoral process originating in the intestine and similar to the secretin mechanism.

Harper and Raper (1943) found that after extraction of the intestinal mucosa with alcohol and removal of the secretin by Mellanby's bile salt method another active substance could be precipitated by saturation of the supernatant fluid with sodium chloride. Purification of this material resulted in a histamine-free substance. Investigation of its action showed that it did not by itself have any effect on the volume of pancreatic juice secreted, but that when injected during a continuous secretion evoked by secretin it markedly raised the concentration of enzymes in the juice; it was therefore called Pancreozymin. This action was not affected by section of the pancreatic nerves or by atropine.

While the zymogen granules in the pancreatic cells are unaffected by injections of purified secretin, they are depleted under the action of pancreozymin, just as under vagal stimulation; further, the oxygen consumption of the gland is raised when pancreozymin is used as an excitant, as occurs also when the gland is stimulated through the vagus. In some respects, therefore, its effects are similar to the results of nervous stimulation of the pancreas. Pancreozymin appears to have a similar distribution in the intestinal mucosa to that of secretin itself. It is discharged into the blood by the presence in the intestine of water, saline and various food substances.

It therefore appears that there is a complete physiological hormonal stimulation of the pancreas from the duodenum and upper small intestine, two different substances being involved: secretin, stimulating the flow of water containing bicarbonate; and pancreozymin, stimulating the discharge of the pancreatic enzymes.

Bile Flow

The pancreas is not the only gland affected by secretin. Following the discovery of this hormone, the fact, shown for the first time over twenty years earlier, that the introduction of acid into the duodenum was followed by an increase in

the rate of bile flow, was amply confirmed. This has lately been demonstrated in man. This indirect stimulation by acid showed that its effect was quantitatively parallel to the distribution of secretin in the intestinal wall. While the earlier experiments were carried out with a mucosal extract containing a depressor substance, it later became clear that secretion by the liver could undoubtedly be stimulated by preparations of secretin which contained no histamine and no bile salt. This also has lately been shown in man. Secretin is not as effective a bile flow stimulant as bile salts themselves ; and there is some evidence that it is mainly the fluid portion rather than the solid content whose output is increased by its action, though this has been questioned.

A striking difference between the liver and the pancreas in this connection is in the latency of the secretin effect. While an injection of secretin usually promotes the flow of pancreatic juice within a minute, it is several minutes before the flow of bile from the liver is augmented. Removal of the intestine and clamping the pancreatic veins before the injection of secretin appeared to show that the cholagogue action of secretin was dependent on one or both of these organs, one hypothesis being that the liver was stimulated by metabolic products from the pancreas when the latter organ had been stimulated by the secretin. However, later experiments in which secretin was injected after removal of the pancreas and the intestine show that the hormone may act directly on the liver, or at any rate independently of these other organs.

The passage of acid into the duodenum was also early shown to produce an increase in the rhythmical tonus waves of the gall-bladder and the evacuation of some of its bile. During the period when the attempts to purify secretin were being most actively pursued it was shown that food and acid in the duodenum affected in this way not only the denervated gall-bladder *in situ*, but also a transplanted bladder, as well as one in a cross-circulated animal. There was thus no doubt about the existence of a humoral mechanism.

It was found, however, that while hydrochloric acid in the duodenum was the most active stimulator of the flow from the pancreas various other substances, particularly fatty acids and fats, were even more powerful in initiating gall-bladder contraction. Boyden's well-known meal of five egg-yolks and half-a-pint of cream was found to be the most powerful way to stimulate evacuation of the human gall-bladder. Intravenous injection of fats and the products of their digestion was shown to have no effect on the gall-bladder, and these substances, therefore, presumably acted, not directly after absorption from the intestine, but indirectly by liberating from the intestinal wall an active substance which passed to the gall-bladder by the blood stream and raised the tone of its muscle.

It gradually became apparent, particularly from the work of Ivy and Oldberg (1928), that in the process of purifying secretin extracts one could obtain preparations which would stimulate the pancreas and not the gall-bladder, and others which would stimulate the gall-bladder and not the pancreas ; and so these authors introduced the name Cholecystokinin for the new hormone which evoked gall-bladder contraction. Its distribution in the mucous membrane of the digestive tract is similar to that of secretin, though a little has been found in the stomach. When it is injected intravenously there is a latent period of about a minute ; the intracystic pressure rises to a maximum in two or three minutes, then slowly falls to the basal level within 15 to 30 minutes.

Secretin and cholecystokinin appear to be closely related substances, though having different isoelectric points (*p*H 7·75 and 5·0–5·5 respectively). The action of neither of them is affected by atropine. Cholecystokinin is also, like secretin, inactivated by an enzyme in the blood plasma. It is significant that no cholecystokinin has been found in the horse, an animal which does not possess a gall-bladder. Cholecystokinin extracts, in addition to their action on the gall-bladder, have been found to increase intestinal motility and may lower the tone of Oddi's sphincter ; if this latter action can be substantiated it will be an interesting example of a reciprocal effect on the biliary pathway, by which not only is bile expelled by the gall-bladder but its passage into the duodenum is also facilitated.

The intestinal motor factor which can be extracted from the mucosa is not histamine, acetylcholine or secretin ; it is not yet certain whether it can be regarded as distinct from cholecystokinin.

Small Intestine

Secretion by the small intestine can be stimulated by a large number of chemical, as well as mechanical, factors from within the intestinal lumen, and it was natural that a possible humoral mechanism should be investigated. The insertion of acid or food into an intestinal loop has been found to stimulate secretion by another loop. Intravenous injection of intestinal mucosa extracts has a similar effect ; so there is a possibility of the existence of a hormone.

Various difficulties have been encountered in the investigation of this problem. While intestinal loops secrete when substances are placed in them they frequently do not do so when food is introduced into another part of the small intestine. However, Nasset (1942) claimed to have found that some hours after administering a mixed meal to dogs he obtained an increased output of enzymes from autotransplanted intestinal loops and postulated the existence of a hormone originating in the mucosa, which he called Enterocrinin. Peptone seems to be the most effective substance for causing its discharge into the blood. The active agent is contained in extracts of the small and large intestine mucosæ, but it is not found in the stomach. It is interesting that the stimulation of enzyme production by this mechanism occurs only when the loops have been deprived of their extrinsic innervation, and there is often but little increase in the volume of juice secreted.

Enterocrinin is distinct from histamine and secretin. Whether it will be possible eventually to regard it as a true hormone mechanism must await extension of the experiments so far carried out.

While the innervated small intestine in general is not easily stimulated by a humoral mechanism to secrete, the duodenum appears to behave differently. Nearly twenty years ago it was shown that the first part of the duodenum, whether denervated or not, secretes mucus when a meal enters the intestine ; and when hydrochloric acid is placed into a duodenal fistula the local secretion which follows is accompanied histologically by a depletion of the mucin content of the cells of Brunner's glands. This effect was at first supposed to be due to secretin ; but since it has recently been found possible to make secretin preparations without this activity (though not as yet secretin-free preparations with it) it may be that a separate hormone exists ; at all events Grossman (1950) believes this to be the case and has given it the provisional name of Duocrinin.

Reference has already been made to the possible existence of an intestinal hormone which stimulates contractions of the small intestine muscle coat. It has been known for the past 30 years that the villi undergo great activity when chyme comes into contact with them. This is due to the rhythmical contractions of the muscle fibres projected into them from the muscularis mucosæ. Kokas and Ludány (1933) claimed that this activity could be brought about, consequent on the admission of acid into the duodenum, in a jejunal loop removed from the abdomen and viviperfused from the animal's carotid artery. As they were able to separate the active factor in the mucosa from histamine, choline, secretin and cholecystokinin, they gave it the distinctive name of Villikinin. The evidence for the existence of this substance has not yet been confirmed by others, and we must therefore regard it with an open mind.

In this brief survey an attempt has been made to give an account of the main features of the intrinsic hormonal processes at present known to be concerned in the physiology of the digestive tract. These hormones certainly form an interesting group. They arise in the alimentary canal and act solely upon it and other structures connected with digestion ; they have no known physiological actions on other organs. There is apparently no synergism in the narrow sense or, with one exception (gastrin and enterogastrone), direct antagonism between them. The mechanisms of which they form part are set into operation by fairly specific stimuli acting on the mucous membrane of the stomach or intestine, in most cases producing an effect either locally or at some adjacent

point in the digestive system. In two cases, enterogastrone and the so far unidentified intestinal excitant of gastric secretion, the action is a retrograde one, i.e. on a part of the tract from which the specific stimulus has already passed.

As already pointed out, the physiological excitants of the digestive glands are of fundamentally different types. The salivary glands are stimulated solely by their nerve supply ; the stomach by both nerves and hormones ; the intestine and its glands probably by hormonal mechanisms alone. The pancreas is perhaps the organ about whose hormonal stimulating mechanism we have at present the clearest conception ; it is interesting how improved methods of purifying secretin extracts caused a temporary setback owing to the inability of secretin itself to evoke the production of a complete pancreatic juice, but at the same time paved the way for the discovery of the complementary hormone, pancreozymin.

This address has dealt mainly with the actions of the gastro-intestinal hormones, and not with the details of methods used for their extraction and purification. It is obvious, however, that the disentangling of one hormonal process from others must depend on obtaining the various substances in a relatively pure state. This is also essential for the deliberate use of the hormones for diagnostic and therapeutic purposes, a practice which commenced a few years ago and seems certain to extend. There is plenty of scope for further study of the physiology of these substances ; the investigation of the actual cells of origin of each, for example, has barely commenced.

In spite of the relative lack of knowledge regarding some of the digestive hormones, particularly those concerned with processes in the intestine, and the need for much further investigation of them, it can justifiably be said that the study of these substances has already revealed the fascinating nature of the chemical methods of co-ordination which are at work in the digestive system.

References

In an address of this scope it is impracticable to quote either the names of all those who have contributed to our knowledge of the gastro-intestinal hormones or even an abbreviated list of references. References to most of the literature on the subject can be found in, amongst others, the following book and reviews :—

Babkin, B. P., 1950 : *Secretory Mechanism of the Digestive Glands*, Second Edition (Hœber).
Ivy, A. C., 1930 : *Physiological Reviews*, **10**, 282.
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HUMAN AFFAIRS AND THE PSYCHOLOGICAL POINT OF VIEW

Address by

DR. J. C. FLUGEL

PRESIDENT OF SECTION J

THE PSYCHOLOGICAL POINT OF VIEW

THE Oxford Dictionary defines 'point of view' as 'the position from which anything is viewed or seen, or from which a picture is taken; also the position or aspect in which anything is seen or regarded.' A point of view as a 'position' or 'aspect' is thus distinct from the objects seen or pictures taken from it; and the point of view of a science can be distinguished from the data or findings of that science. The point of view remains the same whether the things seen from it are few or many, near or distant, blurred or distinct, simple or complex, as we realise if we contemplate the same prospect from the same position on a clear and on a foggy day. Similarly, the prospect from the point of view adopted by a science may be clear and extensive or indistinct and narrow, according to the progress made by that science up to the moment of our contemplation. There must, of course, be a prospect of some sort in order to justify us in speaking of a point of view at all, and when we think of a point of view it is hardly possible to avoid altogether some implied reference to what can be seen from it. Nevertheless, there is perhaps some justification and profit to be derived on occasion from considering the point of view of a science independently, so far as may be, from the actual achievements of that science. This is, I venture to suggest, particularly the case as regards psychology at the present stage of its development. The 'picture' afforded by contemporary psychology is, as we all know, in many respects lamentably blurred and incomplete when we compare it with the wide, clear prospect of the vast territory which we hope will eventually be at our disposal, but yet the mere attainment of the vantage point from which the psychologist looks forth is not without its significance for human development and welfare.

To talk of the point of view of a science obviously implies some understanding of the nature of the science in question and might even, if we are pedantic, seem to require some definition of it. To define psychology, however, is notoriously difficult, and I propose here to circumvent this difficulty by assuming for our present purpose that psychology is all that has been called by this name—in our common speech, in the standard text-books bearing the words 'Psychology' or 'Psychological' upon their title pages (many as is the number of these books and varied as is their method of approach), and in the comprehensive summaries of the literature that are provided in such periodicals as *The Psychological Bulletin* and *Psychological Abstracts*. As such, psychology includes a vast field bordering on many other disciplines. But it includes, above all, the study of both mind and behaviour and the use for these purposes of any methods that seem hopeful or appropriate, as long as they take reasonable account of the ordinary canons of scientific procedure.

The question of method is an important one and is connected with special difficulties and crises of development in the history of psychology. The hope of applying to the mind some of the exact techniques which had proved so spectacularly successful in physical science led, under the influence of Fechner and Wundt, to the momentous introduction of the experimental method and the creation of the psychological laboratory. On the

other hand, the difficulties of dealing scientifically with such elusive phenomena as those of consciousness and character have also led, ever and again, to revolts based on impatience or despair. F. J. Möbius wrote of the 'hopelessness of all psychology,' J. B. Watson wanted to exclude all reference to mind in what he hoped would become the more exact science of behaviour, while Spranger and other exponents of *Geisteswissenschaft* in Germany suggested that psychology should be content with the modest aim of description, abandoning the more ambitious one of explanation, which they held was inapplicable to this sphere. Even Janet and Freud and, following them, almost all modern psychopathologists, while in turn revolting against the materialistic limitations of the older psychiatry, have only been able to maintain their interpretation in terms of mind by immensely enlarging their concept of the mental to include a vast and ill defined region of the 'unconscious.' Nevertheless, even when the differences between the unconscious and the conscious are being stressed, the former is constantly interpreted by analogy with the latter, which, it is implied, affords the only (or at any rate for the present for the most hopeful) clue for understanding those dark regions which are inaccessible to introspection; while behaviourists on their part, though struggling hard to avoid reference to what are to them the unwelcome facts of consciousness, are, as their critics have pointed out, often guilty of backsliding, if in nothing else at least in the necessity they find of expressing in cumbrous roundabout phrases what could much more naturally and easily be described in terms of conscious processes. Meanwhile, the vast majority of psychologists have felt themselves compelled to accept consciousness as a fundamental (indeed in many respects the most fundamental) subject of their studies, inasmuch as the behaviour of human beings, and of at least the other higher animals, can most easily be understood in the light of that connection between our thoughts and desires on the one hand and our actions on the other which we all of us are constantly observing in ourselves.

A belief in the real existence and great importance of the mind is thus (with the possible exception of a few protesting extremists) a common quality of all psychologists and a fundamental factor in determining the psychological point of view. Now it is interesting to note that, in this belief, psychology has something in common with primitive thought. Although contemporary anthropologists may stress the importance of animism less than did those of the preceding generation, there can yet be no doubt that the tendency to interpret the visible and tangible world in terms of mental and spiritual forces is a very significant characteristic of primitive peoples, as it is indeed of all those who have not come under the influence of the machine age with its basis in physical science and its thought in terms of mechanical causation. Turning to our own children, it may be that Piaget, in his pioneering investigations in this direction, was guilty of some exaggeration when he suggested that animism was a mode of thought which inevitably and rather exclusively characterised a certain stage of mental development. Nevertheless, there is good evidence that, though notions of purely physical causation may begin to appear at quite an early age, the attribution (even if half playful) of desires and strivings to inanimate objects is far more common with children than with civilised adults, so that Valentine could say 'there is no need to dwell on the predominantly animistic thought of most children of even five or six when interpreting material events.'

It is indeed clear enough that in mental development, both in the race and in the individual, the sphere of animism tends to become progressively and drastically reduced, while that of material causation becomes correspondingly enlarged. Extreme behaviourism attempted to carry this tendency to what might seem to be its logical conclusion, excluding mind even from the doubtful privilege of being an epiphenomenon which nineteenth-century materialism had allowed it. This apparently ruthless logical consistency might indeed seem to be the strongest *a priori* argument in favour of behaviourism. But most psychologists consider that this attempt has failed, and they persist in claiming for mind at least a small but important corner of the Universe, manifested

directly in their own consciousness and inferentially in the behaviour of their fellow men and of many, perhaps all, of their fellow animals.

But psychology did more than merely preserve the idea of mind and motive as a relic of a once far wider range of animism. If it had done only this, it would merely have kept us on the road of common sense (for just as idealist philosophers when they are not philosophising believe in matter, so also no doubt do behaviourists outside their laboratories believe in mind). But a further and most important step was taken. In endeavouring to become a science, psychology, like any other science, took an interest in its subject for its own sake, not for any ulterior motive. In this it differed from the vast majority of more primitive animistic interpretations, in which the interest lay in the consequences (ultimately in the consequences to ourselves) of the function of real or supposed spiritual agencies, rather than in the nature and working of these agencies themselves. Here as elsewhere, the aim of ' pure ' science is the ascertainment of truth—truth without reference to our hopes and wishes other than those involved in the search for truth itself. And this in turn imposes a hard discipline upon our minds : the abandonment of every form of wishful or ' autistic ' thinking and the whole-hearted acceptance of what Freud called the ' reality principle.' In terms of other psychological schools, this involves a ' determining tendency ', a ' set ' or ' attitude ' in which our cognitive powers are enjoined to work their hardest and for this purpose rendered free, so far as may be possible, from any distorting influence from the orectic sphere. Even in ' applied ' science, this attitude still holds, except that the ultimate aim is not the search for truth for its own sake, but the desire to achieve some practical end, for the attainment of which a further knowledge of the truth may help us. Applied science endeavours to discover the means to this end, but in this endeavour again discards all wishful thinking and works entirely according to the reality principle, allowing the cognitive powers full and untrammelled sway. The engineer may desire to build a bridge, a motor car, an aeroplane, the physician seeks to cure or prevent disease, the psychologist may aim at ascertaining the best method of learning or of reducing industrial fatigue or discontent, but in so far as they are applied scientists, they all accept for these purposes the outlook and methods of their respective pure sciences.

The abandonment of wishful thinking that is implied in the adoption of the scientific point of view has in every sphere had important consequences beyond the mere increase of useful and interesting knowledge in which it may result. In the physical sciences it implies the abandonment of that other great system of thought which, along with animism, is so characteristic of primitive beliefs and the behaviour based upon them—the system of magic, a system which reflects what in psycho-analytic jargon is called the ' omnipotence of thought ' (or, as it should be more correctly termed, the omnipotence of wishes). In psychology the consequences are perhaps particularly significant, partly because psychology in its modern developments is still a relatively new science, to the existence of which society has as yet hardly had time fully to adjust itself, partly, and no doubt principally, because human behaviour is a sphere in which we find it particularly hard to adopt a purely cognitive and unemotional attitude. Our passions, our prejudices, our moral valuations, are liable to interfere more seriously here than in the contemplation of happenings in the non-human and material world ; hence the adoption of the scientific, cognitive point of view in this field may be at once more difficult and more pregnant with possibilities of social import than is the case with the point of view of any other science. It is especially for this reason that I have ventured to suggest that the significance of this point of view for human affairs generally may be worthy of some special study.

The Significance of 'Set'

Before we consider this significance in greater detail and in relation to certain special fields, it may be well, as psychologists, to remind ourselves of the psychological reason for the sobering effect which

the adoption of the objective, scientific, point of view has upon our feelings and our passions. The ultimate reason must surely lie in the phenomenon of 'span' or 'narrowness of consciousness' or 'universal mental competition', as Spearman prefers to call it. In virtue of this competition, cognition tends to drive out conation and affection—just as the emotion connected with prejudice and bias paralyses our judgement and our reason. As a member of Parliament once expressed it, with perhaps unfortunate emphasis on physiological metaphor, 'Learning is apt to produce a swollen head at the expense of weak bowels,' which is complementary to the Nazi injunction to 'think with the blood,' by which is meant that the adherents of that regime should deliberately encourage their emotions to swamp their intellect. And this reminds us that in the last resort conation determines cognition. It is either the arousal of spontaneous interest or emotion, or the more or less deliberate adoption of a 'set,' which induces us to assume a predominantly cognitive or a predominantly orectic attitude. Conation having thus determined the nature of our attitude, the phenomenon of span ensures that different or opposing attitudes shall be excluded for the time being.

The adoption of a predominantly cognitive or 'scientific' set is one of the great achievements of mankind, which has permitted it to think and act more closely in accordance with the reality principle than would otherwise be possible. The discovery and the demonstration of the importance of set is in turn one of the triumphs of experimental psychology and, within the narrow, somewhat artificial field of an experimental situation, it has amply shown how set can control and determine our feelings. Thus, to mention a few examples, Wohlgemuth has demonstrated that the disagreeable feelings aroused by painful stimuli can be enhanced by directing attention upon the observer's experience as a whole, while they can be diminished or even abolished in at least three ways : first and most obviously by directing attention elsewhere, secondly by attending to the cognitive aspects of the experience (e.g. the variations in the quality, intensity and location of the sensations concerned) ; thirdly (and at first sight paradoxically enough) by endeavouring to attend to the feeling as such, as distinct from the total experience of which it forms a part. In these experiments set worked only by determining the direction of attention. But other experiments have demonstrated that it could be equally effective by determining the nature of the orectic attitude towards a stimulus. Washburn and Grose, and following them Yuan Pan, showed that, if we adopt a positive 'seeking' or 'welcoming' attitude towards stimuli which we usually find unpleasant, the whole nature of the affective side of the experience can be remarkably changed, so much so indeed that their results (taken in conjunction with the well-known facts of sexual masochism, as manifested for instance by certain flagellants) almost make credible McDougall's suggestion that the Christian martyrs may have enjoyed their martyrdom, if they welcomed and embraced it as a means of salvation and of testifying to the greater glory of their God. The full implication of such experiments on the field of social attitudes has not yet been worked out. Here too, however, it has been shown that attitudes towards, say, members of a given race, can be greatly changed by appropriate experiences, e.g. by meeting members of that race under favourable circumstances or by obtaining a better understanding of their way of life, their difficulties, aspirations or ideals. Here the change is produced by irradiation of affect or 'conditioning,' not by the deliberate adoption of a 'set' ; but further study would surely show that transition cases of a half voluntary, half involuntary change of attitude may, and do in fact, occur.

These researches, ranging from momentary artificial conditions in the laboratory to matters which may be of permanent social and political importance, have been mentioned as illustrating the significance of changes in attitude which may be brought about by set. In seeing things from what we have called the psychological point of view, we deliberately adopt a cognitive set in which our aim is to study without bias the mental life of others and ourselves, and here the cognitive set will tend to reduce the potency of affect in

much the same way as occurs with the adoption of a similar set in simple laboratory experiments.

Conflicts and Interactions with other points of view : Philosophy

The adoption of the psychological point of view not only has a positive function in enabling us to make a new approach to many problems and thus enlarge our knowledge in all those fields in which mental phenomena play a part. It also contrasts with, and to some extent conflicts with, other attitudes ; and a study of these conflicts is helpful, indeed perhaps essential, to an appreciation of the full significance of the psychological point of view in its bearing on human knowledge and behaviour. It seems, therefore, justifiable to devote the remainder of this address to a brief survey of some of the chief fields in which this conflict manifests itself.

Historically, psychology is to a large extent the child of philosophy and, like so many other parents, philosophy has sometimes been loath to recognise the growing independence of its offspring. It is true also that the problems of psychology are, not only historically but in their very nature, a little closer to those of philosophy than is the case with the physical sciences, and the disputes as to the possibility and justification of psychology as an independent science that were rampant in philosophical circles about the turn of the century have died down rather than been satisfactorily settled. The deciding factor has undoubtedly been that of method. In its insistence on observation and experiment and, where possible, measurement, psychology resembles the physical sciences far more than the parent discipline, for which the proverbial armchair and desk have so far remained the all-sufficient apparatus of study. It is primarily on this basis of a difference in procedure that psychology has won its right to recognition as an independent discipline quite distinct from those of metaphysics, epistemology and logic to which it can so largely trace its origin. As regards logic, however, it has enriched the parent body. The traditional formal logic was far too intellectualistic and ignored the emotional basis of much of our thinking, as a comparison of such books as Thouless's *Straight and Crooked Thinking* with any of the older text-books must, I think, convince us. A vast amount of work, however, still remains to be done in this field. From Freud's *Psychopathology of Everyday Life* and Bleuler's *Autistic Thinking* to the recent American studies of prejudice and ' stereotypes,' psychology has made an enormous contribution to the ways in which human thought is in practice disturbed by orectic factors, while if we accept Spearman's view that error consists in the last resort of the use of a reproductive process where an eductive one is called for, it has made an important step also as regards our understanding of the purely cognitive aspect of the mental acts involved. To put it at the very least, all this implies a very considerable revision and extension of the section on Fallacies in our treatises on logic.

Medicine, Materialism and Determinism

Medicine may be regarded as the second parent of psychology, and it is in this field particularly that psychology conflicts with the materialistic point of view, as exemplified in the physiological and anatomical approaches. Here psychopathology has had to make its way against the view that the ills of the mind should be explained and treated in terms of the body, where neurologists, ' brain specialists,' and more recently endocrinologists, felt they could move on safe and scientific ground. It was, of course, not always so. In its earliest developments, the study of mental disease had to fight rather against crude animistic theories of ' possession,' and the materialistic attitude based on growing medical knowledge of the body and its functions was a healthy reaction against an all too primitive psychological interpretation. But great as has been the progress of knowledge on the physiological side, many aspects of mental disease remained inexplicable in purely somatic terms, and the labours of the psychopathologists have now firmly established the justification and utility of the psychological point of view in medicine.

The great ultimate problems concerning the relation between body and mind are, however, still unsolved and seem likely to remain so. The conflict of psychology with materialism in medicine and elsewhere is only an aspect of the abiding struggle between the opposing points of view of vitalism and mechanism, free will and determinism. The new science of psychosomatic medicine is wisely for the present confining itself chiefly to the study of the correlation of bodily and mental functions rather than attempting the more ambitious task of determining what is cause and what effect in the body—mind relationship, while as regards treatment it encourages a somatic, a mental, or a combined approach, as circumstances seem to indicate or to permit.

This same pragmatic attitude is manifested in the approach of the psychologist and psychopathologist to the problem of determinism. As regards this ancient problem, psychologists might perhaps quite correctly be accused of seeking both to run with the hare and to hunt with the hounds. As a discipline working with the same general concepts and methods as those of physical science psychology is deterministic in its assumptions, in the sense that it does not consider mental events to be arbitrary or unpredictable—such arbitrariness as they may show being, it is assumed, due entirely to the limitations of our knowledge in an admittedly very difficult and complex field. On the other hand, psychology has also of necessity to deal with desires and motives having reference to a future state and therefore in their essence teleological, and with acts of choice and voluntary decision, which are the very phenomena which give rise to the notion of free will. It is round this point that there raged ' the battle of behaviourism,' though McDougall, the protagonist on the anti-behaviourist side, certainly would not agree that by admitting teleology he was depriving psychology of the status of a science. Freud, by his denial of chance or accident in mental life, sought to bring mind more safely and completely within the sphere of scientific determinism, but the determining factors that he found were, once again, motives and ' wishes,' even though unconscious ones. Moreover, in so far as genetics may succeed in demonstrating clearly the inheritance of mental qualities on the orectic side (as it has done already as regards our cognitive endowment), we shall have to reckon more seriously than we often do at present with biologically determined differences in temperament and instinctual endowment, of a kind that are as much beyond our conscious control as are our inherited bodily characteristics —though these differences will still be psychological in nature.

From these developments there would seem to be two main consequences of great importance for the influence of psychology and of the psychological point of view. In the first place is the assumption that there can be a science of motives, whether conscious and introspectible or unconscious and inferred ; that motives, in spite of their teleological nature, show sufficient regularity and predictability to be amenable to study by the general methods that have proved so successful in the physical sciences. In this assumption psychology has in practice quietly short-circuited the old problem of determinism and free will without attempting any serious direct attack upon this problem itself, which it still leaves to the philosopher. In the second place is the acceptance of the view that the sphere of the mental and psychological is wider than the sphere of the conscious, and that, in consequence, there are aspects of mental life which are beyond the control of conscious will and choice and can at best only be indirectly influenced by these. In this the mind is very like the body ; the striped muscles will carry out the orders given by our mind, but all the other functions of our body are at most indirectly under our control. The conscious ego may determine our way of living at the highest level, but for the rest we must admit we are ' lived ' by biological processes that we can influence only by roundabout and often only partially successful methods. All too often both our bodies and our minds behave in ways that consciously we find regrettable.

This last aspect of the psychological point of view, with its implied parallelism between the control of our mind and of our body, has an important bearing upon our

attitude to the ills of the mind—a bearing which, when more generally appreciated, may help to remove the stigma of mental disease, which at present causes additional misery to those afflicted and which may prevent them from seeking treatment at an early stage when it might be most effective. With a few exceptions, bodily illness is relatively respectable, largely for the reason, it would seem, that we are reconciled to the limited control that our minds have over our bodies ; hence we can suffer bodily ailments and still retain our self-respect. We are, however, much less reconciled to the fact that the deeper recesses of our minds are almost equally outside the control of our conscious wishes, This, as Freud once remarked in a famous passage, is a blow to our vanity which we can ill tolerate ; and in consequence we have in mental disease to bear a burden of uneasiness and shame which is in some ways greater than that which attaches to our bodily disorders. But, just as men have survived the earlier traumata which Freud recalls in this connection—those inflicted by Copernicus and Darwin respectively—, so also, no doubt, they are slowly becoming reconciled to this third and most intimate blow to their self esteem, and able to realise that the human ego can still be healthy and vigorous though it is not omnipotent even in the sphere of mind. And just as the progress of physical and biological knowledge provided some compensation for the earlier humiliations, so also may be the case with the increase in our knowledge that has resulted from the studies of our deeper mental processes. If the control of the conscious will is limited, modern psychology at least gives in return some inkling of the forces we are up against. We know something, too, of the way in which our conscious judgement is liable to be distorted by forces that are outside its ordinary field of vision, so that, even if we retain many of our prejudices, we can at least wear them with a somewhat better grace and should be able to admit without undue shame that we may sometimes be mistaken.

The difficulty we have in frankly facing the fact of mental disease as such is, of course, reinforced by the distaste felt for the revelations made by psychopathology concerning the nature of many of the mental tendencies removed from conscious control. The psychology that issued from the studies of the philosophers and the laboratories of the pioneer experimentalists might often seem inadequate, dull, or even pettifogging, but it was hardly ever shocking. That which came from the psychoanalysts' consulting rooms was, however, often profoundly disturbing to our moral and aesthetic sensibilities. The cries of horror that it raised have now considerably abated, and the educated western world has largely accepted many of the unwelcome findings from this quarter. And this in turn is responsible for a slowly increasing sense of psychological realism, even in our more official attitudes and institutions. We know that man is not logical or virtuous throughout his nature, that reason and moral control exercise only a precarious ascendency at the highest level, and that the task of mental and social hygiene consists largely in finding a compromise between crude asocial expression of primitive impulses on the one hand and crippling and dangerous inhibition of our fundamental energies upon the other. It is vitally important for this purpose that we should as far as possible distinguish between taboos (perhaps outworn and irrational) and true socially disruptive behaviour, and psychology is both forcing us and helping us to make this distinction. Here we get the first indication of a new kind of conflict, a conflict between the psychological and what might be broadly termed the moral point of view—a matter to which we shall return in a moment.

Economics

The struggle with a materialistic or mechanistic outlook which psychology had to wage in medicine before it could justify its own point of view is found again —with significant variations—in certain other fields. One of these is economics. Economics could, of course, not dispense altogether with psychology, since it is essentially concerned with the behaviour of human beings as producers and consumers. For its purposes, however, it adopted from nineteenth-century psychology a rather crude rationalist-hedonist

conception of the mind, which, permissible as it might be for a preliminary simplifying of difficult economic problems, is very far from doing justice to the actual complexities of human nature. In this vastly important sphere there is an urgent need for the wider adoption of the psychological point of view, and for the collaboration of economists and psychologists to make this possible. How great is the deficiency in this respect may be inferred from the beneficent inroads of psychology into the field of industry, which after all is only a branch of applied economics. It was industrial psychology, with its insistence on the 'human factor,' which revealed the error in such crude assumptions as that output would be directly proportional to hours of work, that breakages and accidents were purely 'accidental,' and that labour turnover and sickness absenteeism were dependent only upon economic or medical conditions. A more general recognition of the importance of this human factor in the other economic fields of distribution, consumption, saving and spending, will surely prove no less illuminating. To give a single illustration, it may well be that the psychologists' experiments on choice, which in their laboratory setting may seem very academic and removed from the practical affairs of life, may nevertheless be capable of throwing useful light on the way that millions of people spend their money daily in thousands of shops throughout the world.

Education and the Conflict with the Moral Point of View

Turning to education, we find that here also the adoption of a psychological point of view has drawn attention to neglected aspects of the human factor—in this case as regards the learning process. It has, for instance exposed the vast oversimplifications and distortions inherent in the classical views regarding formal training, on which so much of educational theory was based, and in a multitude of other ways has thrown light on the complex problems of acquiring skills and knowledge and of developing the character qualities which permit us to become useful and pleasant members of society.

At the same time it is in this field that we get a further hint of that conflict between the psychological and the moral point of view which we already noted in connection with the findings of psychopathology. To some extent of course, education is necessarily a moral process, inasmuch as the behaviour it demands often runs counter to our primitive desires. But to the psychologist with his cognitive (as opposed to orectic) approach, it often seems that this inevitable moral element has been unduly stressed, with much unnecessary guilt and suffering as consequences. This is especially the case when, as a result of such moral overemphasis, reliance is placed on more or less arbitrary rewards and punishments as the incentives appropriate to the educative process. Moreover, such an attitude encourages anxiety and deception in the learner and a smug and, all too often, cruel, complacency in the teacher. In the past the field of education was often a happy hunting ground for sadists. Buttressed by an outlook which tended to attribute all inefficiency in learning to moral weakness, they could gratify their own aggression and pleasure in the infliction of pain without any qualms of conscience, since punishment was supposed to be inflicted for the moral benefit of the victim—a rationalisation of cruelty which we meet again in even more sinister forms in other spheres. The tendency towards a relative amoralisation of education owes much to psychology, and while some modern experiments in 'free' education, in their reaction against a former moral oppressiveness, may have gone further than many psychologists would approve (since they seem to place too great a strain upon the individual conscience and may provide an inadequate preparation for the inevitable restrictions on freedom in adult life), there is yet little doubt that the movement as a whole is a healthy one, due to a large extent to the substitution of a psychological for a too exclusively moral point of view.

Law and Delinquency

When we come to the sphere of law and the treatment of delinquency, we find this conflict between the moral and the

psychological points of view appearing in much more clear and open form. Though morality is wider than law, law is, after all, but codified morality. The primary business of the representatives of law in its various branches is to ascertain whether the law has been broken, to apprehend offenders, and to inflict authorised punishment when guilt has been proved. In this their sphere and outlook are totally different from those of the psychologist, but the psychological point of view begins to infiltrate into, and to complicate, law as soon as motive is taken into account, as is the case with all the more advanced legal systems. A further rapprochement with psychology takes place when the prevention, as distinct from the detection and punishment, of crime begins to be considered. In western legal thought, three main purposes have long been associated with punishment: the reformation of the offender, the deterrent effect on other potential offenders, and the motive of retribution or revenge. From their own point of view, psychologists have very justifiably asked whether punishment inflicted in accordance with more or less fixed and arbitrary rules is likely to be the most efficient method of reformation or deterrence, and their study of the criminal mind has suggested that the main causes of delinquency are to be found in environmental conditions (especially in those of early life) with which the law as such is not concerned. In the more advanced countries, they have indeed succeeded in powerfully influencing legal practice, according to which young offenders, at least, are increasingly ordered treatment rather than punishment; while even with those of maturer age the mental condition of the offender is increasingly taken into account. This success has, however, brought psychology into sharper conflict with those who stress the legal and moral points of view and who are obviously influenced by the revenge motive, which demands that those who have inflicted suffering on innocent members of society should suffer in their turn. Psychologists can hardly neglect the deep psychological foundation of this motive, and indeed the psycho-analysts have brought evidence to show that the notion of punishment, and in particular of 'making the punishment fit the crime,' is profoundly embedded in the human mind and thus more than an arbitrary social institution. They have further shown, however, that the effects of punishment are far from uniform. When accepted as a just retribution, punishment may have a salutary influence; but when not so accepted, it may only confirm the offender in his anti-social attitude, either because he feels that his guilt has been wiped out by punishment or because the punishment has strengthened him in his belief that society is his enemy. Furthermore, as in the educational sphere, the infliction of punishment may often serve as a rationalisation for aggression and sadism or as a vicarious atonement for personal guilt on the part of those who demand it or are, directly or indirectly, responsible for carrying it out. The whole problem of punishment is thus so complicated that psychologists can hardly afford to be dogmatic in their claims, except in so far as they demand the right of being given every opportunity for further study from their own special point of view. There can be no question of the value of this point of view, even though the problem of just when it should on the one hand supplement or supersede, or on the other hand give place to, the opposing legal or moral points of view, is one that is likely to remain unsettled for many years to come.

Ethics and Values

This conflict between contrasting or opposing points of view in the field of delinquency leads us naturally to the question of the relation between psychology and ethics. When I and those of my generation first embarked on the study of psychology in the early years of this century, we usually settled the matter to our own satisfaction by saying that psychology was a positive science, whose business it was to describe and, if it could, explain the phenomena of consciousness and of behaviour, without evaluating them in moral terms; while ethics was a normative discipline, which told us how we should behave, and why. While most present-day psychologists would, I believe, still generally agree with this

distinction, it has to be admitted that the boundary between the two disciplines has become in some respects a little blurred, and this chiefly for two reasons ; in the first place owing to the great developments of applied psychology in medicine, education, industry, criminology and other fields ; in the second place, because psychology has increasingly concerned itself with the mental structures and processes involved in moral behaviour, with the formation of conscience, with the factors in character that determine 'good' or 'bad' behaviour, with the conflict between desire and moral control, with acts of will and moral choice. In its study of the nature and development of conscience, it has shown the relativity of this mental institution (if we may use this term), both as between individuals and societies and within any one individual, inasmuch as conscience is not all of a piece but contains various levels, not all of which are logically or ethically compatible. Some of these levels, we now realise, are crude and archaic and, judged from more civilised and sophisticated standpoints, often unrealistic and harmful in their influence. A vast proportion of man's inhumanity to man is due, not so much to simple selfishness, ruthlessness or lack of sympathy, as to the working of a primitive, narrow or distorted conscience. In thus revealing the complexity and questioning the validity and internal consistency of conscience, psychology has deprived ethics of one of its weapons (the direct appeal to what has sometimes been called the 'moral sense') and has made ethics somewhat dependent on psychology for its verdict on the standing and respectability of any particular moral judgment.

In so far as applied psychology is concerned, psychology, like any other applied science, presupposes certain goals or values, and sets out to find the best means of achieving them. It accepts, for instance, the desirability of mental health, of efficient learning, of satisfaction and success in work. The question of the validity and worth-whileness of these goals it leaves to other disciplines, and ultimately to ethics. Thus at first sight, the distinction between ethics and psychology as an applied science remains clear enough. But in practice certain problems often arise which confuse the picture.

In the first place, we may sometimes have to ask 'Does the end justify the means ?' This may happen, for instance, when we have to decide whether the probable gain in mental health will justify the expenditure of time, energy and money on a lengthy psychotherapeutic treatment. Secondly, and more importantly, it is not always so easy to distinguish between means and ends. There is in reality a whole hierarchy of ends, and what is end and what is means may depend upon a point of view which may vary almost from one moment to another, until, if we pursue the matter far enough, we are driven to assume the existence of some ultimate intrinsic value some *summum bonum*, which is indisputably a good in itself. Here the borderline between psychology and ethics will be a fluctuating one, according to what we accept at the moment to be means or end. In the third place, the study of the means may sometimes modify, enlarge or call in question the nature of the end. Some of the pioneers in industrial psychology, with their emphasis on time and motion study, accepted as we now believe too narrow an end, and the aims of this branch of psychology had to be enlarged to deal with all aspects of the human factor in every kind of work. Furthermore, we may even deliberately seek to produce a change of end within the individual mind, as in the treatment of delinquents and neurotics, who, as the result of increased psychological insight will, it is hoped, modify their aims and ideals in the direction of greater sociality or realism. Such modifications may even apply to large groups, as when we speak of the 're-education of a people' or of 'education for democracy.' In all such cases, applied psychology, in attempting to modify human ends, may be said to overlap with or invade the sphere of ethics, though the psychologists responsible for this are in most cases hardly aware, and would perhaps be unwilling to admit, that they are overstepping the boundaries of their own science.

In so far as the direct psychological study of values is concerned, much depends upon whether we hold, with some philosophers from Plato onwards, that values

exist independently of their appreciation by the mind, or with others, that they are by their very nature subjective. In the former case, they are in the last resort definitely outside the sphere of psychology (though psychology may still legitimately concern itself with the manner of their appreciation). If, however, we consider that values are ultimately in the mind, are states of mind or aspects of states of mind, they fall just as definitely within this sphere, at least so far as the description of their nature is concerned. Endeavouring to preserve the distinction between psychology as a positive science and ethics as a normative one, we might say that psychology should (and to a large extent does) confine itself to description and explanation, avoiding evaluation; but here again the very processes of description and explanation inevitably sometimes affect our estimates of value, as in judgements of good or bad taste, crude or cultivated moral standards, based upon the degree of complexity and refinement of the mental processes involved. Here too, therefore, the boundary between the psychological and the ethical points of view is apt to be a fluctuating one, and psychology may be said to have made some serious incursions into what was formerly regarded as the territory of its sister discipline.

Æsthetics

These considerations with regard to ethics hold largely also as regards æsthetics, though here perhaps the believers in absolute values are fewer and there are many who might even be prepared to admit that æsthetics is no more than a special branch of psychology. Nevertheless, here also the distinction between a positive and a normative discipline holds good for the most part, and in much the same way as in the case of the relation between psychology and ethics. Psychologists may study the processes of æsthetic choice and judgement, they may describe and analyse the cognitive and orectic functions involved, and may even relate them to the personality type, developmental history and 'complexes' of the individual subject, but in the main they endeavour to avoid æsthetic judgements in terms of final evaluations—though here, as with ethics, psychological studies are not without their influence upon such evaluations. Some artists, indeed, have been apprehensive of psychology, fearing that too deep a psychological analysis may deprive art of its apparent value and impair the creative power of the artist himself. At a first view there might seem to be some justification for this anxiety. Art occupies a peculiar borderland between fantasy and artistic thinking on the one hand and full reality thinking on the other. It has thus something in common with neurotic symptoms, and if psychoanalysis or any other psychotherapeutic treatment can remove such symptoms, why should it not abolish the aptitude for artistic creation and appreciation also? It would be idle to pretend that we know the full answer to this question. The evidence available, however, would seem to show that such fears are largely groundless. They arise partly from an exaggerated estimate of the power of psychoanalysis, partly from a misunderstanding of its effects and aims. Psycho-analysis aims at strengthening the ego, and art is for the most part highly ego-syntonic. Psycho-analysis may indeed reduce or abolish activities of which the ego disapproves, among them neurotic symptoms and certain harmful character traits, but it seems highly unlikely that, even if this were its aim, it could deprive us of that impulse to create and enjoy art, which has been so constant and characteristic a feature of the human mind in all societies throughout recorded history.

Religion

The conflict between psychology and religion has been in some respects acuter than that with either ethics or æsthetics, and it is here perhaps more than anywhere else that it has sometimes been felt that the psychological point of view is one that threatens values. Religion, as distinct from ethics or æsthetics, quite definitely and generally postulates the non-subjective nature of values, in the shape of a God or gods who constitute these values and from whom the values recognised by human beings may be said to flow. This puts religion at once both in a stronger and in

a more vulnerable position than the two above mentioned disciplines. In so far as we believe firmly in the existence of a divine being, religion is quite immune from any threat from psychology, which can of course legitimately study the religious consciousness, but can have nothing to say about the ultimate nature of the divine being himself, which remains a matter for theology or metaphysics. But anxiety may arise as soon as we conceive it possible that psychology in the course of its investigations may somehow convince us that religion is purely subjective, merely a particular attitude of mind, and that the divine being is no more than a figment of the human imagination. Such anxiety might find some justification in the fact that, according to one American investigation, there are fewer believers in God among psychologists than among the representatives of most other sciences.

It is true, moreover, that there have been a few psychologists who have held that the increasing light thrown by their discipline upon the way religious beliefs and attitudes develop in the mind has definitely shown that religion is in fact completely subjective in its nature, and that the belief in the objective existence of God is therefore an ' illusion.' This was hardly the view of Freud (notwithstanding his use of this term), nor indeed of any of the more cautious students of religious psychology, from Ribot onwards, who realised that to show that religious beliefs might correspond to deep rooted and primitive human fantasies did not necessarily prove them to be untrue. The answer to the question of the objective truth or untruth of religious dogma, if it can be given in scientific terms at all, can hardly be forthcoming from psychology. Nevertheless, by revealing the ' autistic ' or ' wishful ' basis of much religious thinking, psychology increased the difficulties already attaching to belief in the metaphysical aspects of religion and has thus undoubtedly played its own part—and in recent years an important part—in the general antagonism between science and religion—an antagonism in which geology and biology were perhaps the protagonists on the scientific side in an earlier generation. It is this arousal of scepticism on the ground that religious belief is contaminated by wishful thinking which is the chief factor in the opposition between the psychological and the religious points of view at the present day.

When we turn, however, from religious dogma to the emotional attitudes involved in religion, psychology is at one with the upholders of religion in recognising the great significance of these attitudes and the important part they play in both individual and society. A sense of living in harmony with some principle or purpose that transcends the individual life seems essential for the higher satisfactions and aspirations of humanity. These aspirations must find an outlet somewhere, and organised religion, although in its inherent conservatism it may not fully meet the needs of the more cultured and progressive sections of mankind, may be in many ways a far preferable alternative to certain other outlets which have played a prominent part in recent history.

Politics

Among the most important of these outlets, it is pretty generally agreed, are some within the field of politics, and the political enthusiasms and intolerances displayed in the present century are almost certainly to some extent the modern equivalents of similar enthusiasms and intolerances formerly manifested in the religious field. Here the picture is undoubtedly a gloomy one. The problems involved are at the moment the most pressing of all those that beset humanity, and for their solution, psychological knowledge and insight are most urgently required. Unfortunately, social psychology, in spite of some promising advances here and there, is still lamentably backward as compared with the vast tasks that confront it, while politicians on their side have for the most part shown little inclination to avail themselves of such knowledge as social psychologists possess, nor have they taken serious steps to encourage psychological research on a scale commensurate with the immense issues at stake. In this field, psychology encounters, not so much anxiety or opposition (this stage perhaps has not yet been reached), as indifference and ignorance.

Little as the psychologist may at present

have to offer, it yet surely merits more attention than it usually receives at the hands of those who are responsible for making great decisions involving human destiny. The methods of measuring and influencing public opinion have indeed received some little notice, but most of such refinements of these methods as we possess have been developed and used for *ad hoc* purposes rather than for large-scale systematic research. The existence of 'stereotypes' in social attitudes, often involving gross racial or national prejudices of a very harmful kind, calls out for wider consideration in relation to inter-group conflicts, as do also the means of modifying such prejudices—a matter concerning which psychologists have thrown some light in their still relatively small scale experiments. Surveys of public attitudes can also teach us much with regard to the psychological resistances which impede the measures directed to social and economic improvement which are suggested by other sciences; for instance as regards the great problem of the adjustment of world population to world resources, a matter which occupied the attention of the last meeting of the British Association and which clearly requires for its solution both the widespread introduction of improved agricultural methods and restriction of the growth of population in many overcrowded countries — both of them (especially the latter) being matters which appeal to our prejudices and emotions in a way that greatly darkens counsel. Largely from psycho-analysis we have learned of what seems to be the immensely important psychological mechanism of 'splitting' or 'decomposition,' in which the positive and negative elements in an originally ambivalent attitude are dissociated and directed towards different persons or groups, which then tend to be regarded as crudely 'good' or 'bad.' Hardly less important, and often connected with it, is the mechanism of 'projection,' by which our own faults or those of our group are attributed to others, sometimes with a pathological intensity and disregard of reality which are comparable to those of the individual paranoiac. Connected with this again is the sinister tendency, noted by all social psychologists from Le Bon onwards, for individual conscience and judgement to be abrogated in favour of a childish and irresponsible idealisation of the group or its leader and of everything they stand for—a tendency which in turn may be followed by exasperated disillusion. Psychologists have also clearly recognised a melancholy fact which had for long been dimly apprehended, viz. that intra-group cohesion is most easily produced and fostered by inter-group conflict, though the immensely significant social problem of how to produce maximal internal cohesion with minimal external conflict has hardly yet been clearly stated, let alone approached in the spirit of research. It may well be that, as a number of eminent thinkers from William James onward have suggested, a reasonably satisfactory solution can only be found in some 'moral equivalent of war,' in some concerted direction of human aggression against all those forces of nature (including perhaps some within the human mind itself) which imperil and frustrate the life of man. In all these, and in many other matters, psychologists have called attention to factors which have an intimate bearing on the perils which beset us in the political sphere, but as yet, alas, there is no 'political psychology' comparable to the well-established branches of medical, industrial, or educational psychology. Our so called social psychology has still as yet only superficial contacts with the stern political realities on which our civilisation may so easily be wrecked.

On the more positive side, the recent findings of cultural anthropology, with its largely psychological outlook, have made us aware of the enormous variation in the patterns of culture prevalent in human society, have shown that each such pattern has both beneficent and harmful features, and are thus teaching us to be more tolerant towards cultures different to our own. The demonstration of human adaptability that these findings have provided throw a hopeful ray of light on an otherwise dark scene and have encouraged many psychologists to believe that a solution of man's terribly urgent problems in the social and political sphere is definitely possible, if only the right approach can be found. This approach,

moreover, it is felt, must be made at many levels, from the nursery at one end to the statesmen's council at the other, and it is evident that such an approach requires the combined and co-ordinated efforts of workers in many different fields. This was the attitude adopted at the great International Congress on Mental Health held in London two years ago, and it is the attitude underlying the work of the World Federation for Mental Health which came into being at that Congress. It implies a programme that is, in the terms there adopted, interdisciplinary or interprofessional, and requires that psychologists should act in concert with representatives of many allied disciplines. In all such work, however, we may be sure that the psychological point of view will retain its importance. It is only by adopting an unbiased and cognitive attitude towards the human mind in all its myriad manifestations throughout the many different and sometimes conflicting forms of human society that a task of this magnitude can be attempted with any prospect of success. In politics this point of view is still woefully neglected ; our passions and prejudices still exercise a highly dangerous influence on the relations between human groups. Even apart from actual increase of scientific knowledge, the mere adoption of the psychological point of view, with its calm but not unsympathetic objectivity, would be an important preliminary step towards better understanding between men of different cultures, races, ideologies and nations. In our present crisis we can repeat with a deeper conviction and a greater sense of urgency that ' the proper study of mankind is man ' : and psychology is the most intimate of all the sciences which at the present day make up this study.

Man's Use of Energy

One last word concerning the main theme of this meeting of the British Association : Man's Use of Energy. In every one of its applications, psychology aims at making better use of man's own energy, the energy of his mind and body. It is here, in his own organism, that man's use of energy starts, and it is only through this energy that he can release or modify the energies of the external world. The great progress of the physical sciences has enabled man to use and control these outer energies to a degree which constitutes a grave threat to his own existence if he uses them unwisely, but he can only use them wisely if he can direct to beneficent ends the energies inherent in his own mind. It is psychology which can teach him the nature of these energies and the methods possible for their direction. Hence psychology occupies a key position in the whole problem of the human use of energy. It is incumbent on psychologists that they should fully realise the great responsibilities which fall upon them in virtue of this fact.

MODERN TRENDS IN THE CLASSIFICATION OF PLANTS

Address by

DR. W. B. TURRILL

PRESIDENT OF SECTION K

INTRODUCTION

THE chief purpose of this address is to show, especially to those who have yet to find their main lines of research in botany, how wide and fascinating a field is covered by the phrase ' classification of plants.' It is true that classification, even if termed systematics or taxonomy, is ' a sorting into groups or classes,' but this is only a broad statement which tells one nothing of the purposes, of the principles, or of the process of classification. Recently, in this country at any rate, there have been signs of a renewed interest in the classification of plants and animals. It is of some importance, if such interest is to be maintained and increased, to consider briefly the reasons for the unduly long period during which such studies have been regarded with disfavour by many biologists. First, the taxonomists themselves during the last hundred years have been, with some exceptions, too conservative and too rigid to modify their principles or their methods. Even the wide acceptance of the theory of evolution by natural selection left many of them, in practice at least, where it found them. Secondly, the rise or extension of other branches of botany, physiology, biochemistry, anatomy, embryology, etc., and more recently of cytology, genetics, and ecology, captured the imagination and enthusiasm of students. It can hardly be gainsaid that these subjects are easier to teach, under the average conditions of departments of botany in schools and colleges, than is taxonomy. Perhaps, also, teachers have some difficulty in camouflaging their ignorance in reply to the simple question ' What is this ? ' Thirdly, the work of Hofmeister, and others, drew attention to the groups of cryptogamic plants and the many new problems which could only be solved by working out their life-histories. Such studies were extended by many workers in the *Pteridophyta*, *Bryophyta*, *Fungi*, and *Algae*. Again, one notes that such life-histories can, up to a point, be adequately worked out in laboratories. Fourthly, as a gradual result of the decreased interest in classification and the increased interest in other branches of biology, there developed what was essentially a vicious circle : teachers neglected to teach systematics, students ceased to study it, there were fewer and fewer teachers capable of teaching it, and the circle became complete. It can scarcely be said to have been well and truly broken even yet, but it is being stretched.

It is not possible to give exact figures for the number of species, genera, or families accepted by botanists, mainly because of the diversity of opinion between extreme splitters and extreme lumpers. A reasonable estimate is 250,000 species in the *Spermatophyta*. In the period between the two world wars about 2,000 new species of seed-bearing plants were being described every year. One may hazard a guess that there are somewhere in the region of one million species of plants of all groups at present on the surface of the earth and in the waters thereof. These figures are based on a moderate view of species. Even if they be halved or doubled it will be seen that the problems of distinguishing species and grouping them into larger categories is a task of considerable, but not impossible, magnitude on the basis of number of kinds involved. The figures given, rough estimates though they be, give some idea of the

need for grouping into classes if research of any kind is to be made intelligible to anyone other than the research worker himself.

The difficulties, and the interest, of plant classification are not, however, due to the numbers of species but to the various constancies and inconstancies of characters and character combinations. Any plant classification is 'artificial' in the sense that the botanist determines the status of any group he accepts, and states its diagnostic characters. Any plant classification is 'natural' in that it involves existing natural objects grouped on the basis of determinable characters. 'Species,' for example, occur concretely in nature in that at any one moment there are in existence (say) *x* million individuals having such and such characters and not having others. Individuals, however, come and go, so that there has to be in classification an abstract idea of 'species.' Added to this is the fact of evolution, of changes of characters and of new character combinations from generation to generation.

Present-day Taxonomy

Gross morphology provides the foundation for orthodox taxonomy. Man classified plants, however inadequately, before he invented the compound microscope or even hand lenses, and it was essential he should be able to recognise many kinds from their structure as seen with the unaided eye, either for the uses he wished to make of them, or to avoid them as useless or dangerous. Scientific classification of plants partly grew out of economic botany. Apart, however, from the historical reasons for the emphasis placed on structure in classification it is still true to say that gross morphology provides, in general, the most practical of diagnostic characters for determination of classes within the *Spermatophyta*. This, of course, does not mean that anatomy, cytology, and physiology in the broadest sense cannot contribute additional characters that may be of great value for special purposes or for adding refinement to a classification based on gross morphology. There are, however, obvious reasons why visible characters that demand neither preparation nor delay in their application are most valuable practically. Any theoretical objection to this loses much of its force since one usually finds a high correlation between characters of all kinds. Before considering in some detail the relationship between taxonomy and modern developments in other branches of botany, it is worth while commenting upon the recent improvements in what one may term the orthodox methods.

First, as to specimens and other data. It is fair to say that, on the whole, field collecting has greatly improved. We now preach quality of material, not quantity of collectors' numbers. Greater care is taken with the actual layout of specimens; the material is more adequate, better preserved, and more attractive. Ordinary dried specimens are supplemented more and more by fruits and, for trees and shrubs, samples of wood preserved separately with the same field numbers, and by material in spirit mixture. The information provided on labels is often more detailed and records valuable field observations more precisely. One may advocate that taxonomists should refuse to deal with scraps, unless there be some very strong reason for spending time on them. There is too much scrappy material in the herbaria at Kew and the British Museum (Natural History) and it should not be increased. A good deal remains to be done to bring our sets of specimens up to date in this respect. A few months ago I had to work out the structure and facts in the life-history of *Ceratostigma plumbaginoides*, a widely cultivated plant of the *Plumbaginaceae* from eastern China. Not a single dried specimen at Kew showed the striking underground rhizome system. One could give many similar examples. Winter, or dry season states are rarely represented. Nevertheless, the average collecting to-day is better than it has been and the taxonomist has the opportunity of reflecting this in his publications.

Another general improvement is in describing plants, especially new species and other new taxa. Nowadays, descriptions are fuller and more carefully prepared than they were formerly. They are also often more comparative. Botanical

illustrations also show improvement, especially in black and white dissections of reproductive parts. Attention may be called to Miss Ross-Craig's *Drawings of British Plants*, the *Icones Plantarum*, and the *Botanical Magazine*. It is true that the great rise in cost of reproduction makes the publication of first-class coloured plates almost, or quite, prohibitive, and botanists would welcome research leading to methods of adequate colour reproduction at a reasonable cost.

Nomenclature is too often one of the unavoidable evils of systematics. There have undoubtedly been some improvements of the International Rules of Nomenclature as established at Vienna in 1905. Those of us who regard nomenclature as a tool and not an end in itself, would plead for simplification of rules, and some of us would like to have conservation of names for taxa of all ranks when this would prevent name changes for other than very strong botanical reasons. Another time-saving simplification, with several advantages, is the decapitalisation of the initial letter of all specific and infra-specific epithets, and this has been officially adopted at Kew.

Botanical taxonomy is very directly dependent on herbaria. These continue to grow in size and usefulness. Of the great collections in this country, only that at the British Museum (Natural History) suffered some damage from enemy action during the second world war. Unfortunately, on the continent of Europe there were considerable losses. The greater part of the herbarium and library at Berlin-Dahlem was destroyed. About one-fifth of the herbarium material removed from Vienna for safety was burnt. The new collections at Belgrade were completely lost, as were the old ones in the 1914–18 war. These and other losses lead one to advocate the distribution of co-type specimens to as many herbaria as possible and to urge collectors to prepare duplicates for this purpose. Retypification in place of types no longer in existence is a technical matter which must be considered by taxonomists.

Systematic monographs and floras, or papers preliminary to one or other of these, are the main publications of purely systematic researches. Whether it will be possible for the great series of Pflanzenreich monographs to be continued beyond those completed before the destruction of the Berlin-Dahlem herbarium remains doubtful. Valuable monographs continue to appear apart from this series. Mention can be made here of only a few as examples of rather different kinds. One may select S. Murbeck's full accounts of *Celsia* (Lund, 1925) and *Verbascum* (Lund, 1933), the series of papers on the genus *Primula* by W. W. Smith and H. R. Fletcher (*Trans. Roy. Soc. Edinb.* 1946, 1948, etc.), F. C. Stern's study of the genus *Paeonia* (London, 1946) with its beautiful coloured illustrations, and E. B. Babcock's two volumes on the genus *Crepis* (Berkeley and Los Angeles, 1947). There are many others, but these serve to show that what may well become taxonomic classics are still being produced. There is at least as much activity along floristic lines. Again, only a few examples of recently published important works can be quoted. Marie-Victorin, *Flore Laurentienne* (Montreal, 1935) and W. Robyns, *Flore des Spermatophytes du Parc National Albert*, 2 vols. (Bruxelles, 1947–48), deserve mention, as does the *Flore du Congo Belge et du Ruanda-Urundi*, of which one volume has appeared (1948). Also in course of publication are the two great works dealing with South American floras. F. C. Hoehne, *Flora Brasilica* (1940–) and H. R. Descole, *Genera et species plantarum Argentinarum* (1943–). A. W. Exell's *Catalogue of the vascular plants of S. Tomé*, published by the trustees of the British Museum (London, 1944) is held in high esteem. We look forward to the appearance of the new British flora by Clapham, Tutin, and Warburg. It is also appropriate to wish success to the *Flora Malesiana* under the editorship of van Steenis, and to the *Flora of Tropical East Africa* which has been commenced at Kew.

These brief comments on work recently published or in progress have the double object of proving that plant taxonomy is not static, and of indicating to students with a flair for orthodox systematics that such work is well worth doing. There are many genera requiring revision, and up-to-date floras are needed for many parts of the world.

Taxonomy in Relation to Other Branches of Botany

Plant taxonomy, however, has much to offer to those who, trained also in other branches of botany, may not take kindly to the somewhat arduous, and at times perhaps apparently monotonous, tasks of description and classification isolated from wider problems. Many modern trends in plant taxonomy are towards synthesis between the older methods and outlooks, and more recent developments in our knowledge of plants. Such synthesis has two results of importance to systematics: the production of special classifications and the improvement of general classification by widening the range of criteria and enabling better evaluation to be made of those already in use. It is essential to have some sort of classification at the very beginning of any biological investigation, however incomplete and tentative the scheme may be. It is sometimes convenient to refer to first attempts at systematic arrangement based solely on superficial characters as an alpha classification, and to envisage this being gradually improved, incorporating new knowledge and moving some way along the Greek alphabet to an ideal or omega classification which shall summate all possible knowledge of plant life. An omega classification may never be attained or attainable but we may assume, in all humility, that we are establishing ourselves in, shall we say, a beta position. Let us consider what has been achieved, and what are further possibilities, under a number of headings arranged, to avoid bias, in an alphabetical sequence.

Anatomy and Plant Classification

In some groups of cryptogams structure as seen under the compound microscope is a *sine qua non* of adequate taxonomic description. It is also true that many botanists have used histological data taxonomically in the seed-bearing plants. Yet, as Metcalfe has pointed out, there has been a tendency for physiological and anatomical studies of the higher plants to be separated from the study of their classification. Two generalisations, with exceptions, are relevant. First, stem anatomy, and particularly the structure of secondary wood, has received more attention in this connection than has the anatomy of other parts of the plant. This, of course, is correlated with the economic importance of timbers. Secondly, some families have been relatively well investigated anatomically (as, for example, *Thymelaeaceae*, *Magnoliaceae* and allied families, and *Sapindaceae*), others (as *Sapotaceae*, *Violaceae*, and *Hydnoraceae*) have been much neglected. Systematists who frequently consult the great work of Solereder must welcome the appearance of Metcalfe and Chalk's *Anatomy of the Dicotyledons* which has been prepared at the Jodrell Laboratory, Kew, with the assistance of Dr. Chalk and has just been published by the Oxford University Press. Not only does this book provide up-to-date information on what has been done, but it will undoubtedly stimulate interest in the subject in both its practical and theoretical aspects.

There is no doubt that, particularly at the family level, anatomical characters are correlated to a high degree with those of gross morphology. It is thus often possible by the use of anatomical techniques to determine specimens that are too incomplete for determination by other means and also to obtain additional data for the better classification of plants of doubtful or complicated affinities.

Biochemistry and Plant Classification

It has long been known that the members of certain families tend to have certain chemical properties in common. Volatile oils are characteristic of the *Labiatae*, alkaloids of the *Papaveraceae* and *Solanaceae*, and cyanogenetic glucosides of the *Leguminosae* and *Rosaceae*. Yet these groups of substances occur also in many other families. The matter is of very great interest since the evidence available suggests that biochemical characters very clearly illustrate the phenomena of reticulation and pseudo-reticulation to which reference is made later. Weevers in 1943 outlined some of the problems. The actual occurrence of a given chemical product is by itself no proof of affinity. Thus, the alkaloid caffeine is known to occur in the *Centrospermae*, *Columniferae*, *Gruinales*, *Parietales*, and *Rubiales*, but in only one genus (and sometimes only one

species) of every one of these cohorts. Eugenol (a substance contained in oil of cloves) has been recorded in *Coniferae*, *Myrtales*, *Burseraceae*, *Labiatae*, and *Scitamineae*. Many similar examples of the wide taxonomic distribution of chemical compounds could be given. Closer attention to details, however, somewhat changes the picture. If associations of chemical compounds be considered, and not merely the presence or absence of one, the taxonomic value of biochemistry becomes more evident. Thus, cineol occurs in some 40 families of angiosperms, monocotyledons and dicotyledons. It is, however, a constant constituent of the volatile oils of *Eucalyptus* associated, in various combinations, with about 20 other different compounds. The grouping varies from species to species. Again, the alkaloid protopine is found in nearly all *Papaveraceae* but every genus, and perhaps every species, has its own characteristic associated group of alkaloids. The basic grouping in the *Fumarioideae* differs from that of the *Papaveroideae*.

There is little doubt that very often there is a close correlation between the nature of chemical products and the taxonomy of a species, genus, or family as established mainly on gross morphology. Weevers has pointed out that chemical compounds derived more or less directly from ubiquitous mother-substances, such as lignin, are likely to occur, perhaps as the result of a single mutation, at many scattered points. Those substances and groups of substances whose formation required two or more mutations are much more likely to indicate phylogenetic connections.

McNair's attempts to indicate the degree of evolution of families within climatically similar areas by the properties of their alkaloids, glycerides, or volatile oils, have been severely criticised on chemical grounds. They are also open to objection in that Engler's system is the basis used for comparison, and Engler himself never claimed that his system as a whole was phylogenetic, but only that it showed a series of progressions.

Cytology and Plant Classification

In some groups of the *Algae*, and, to a less degree, the *Bryophyta*, chloroplasts are important in providing taxonomic characters. A comparative survey of chloroplasts in the *Spermatophyta* might reveal more differences than are indicated by the few references in text-books. However, nowadays chromosomes dominate cytological studies and with the very strong evidence that those genes conforming to the Mendelian scheme of inheritance are carried in, on, or by the chromosomes, the taxonomist may expect the number, size, shape, and behaviour of these structures to be characters he must consider. There is no doubt that karyology can throw much light on taxonomic problems, particularly at about the species level. It can do this especially in two ways : by providing additional characters and by indicating with greater or less certainty the origin of this or that taxon. There are, however, several difficulties and dangers.

The taxonomist cannot always rely on the determination of the material used by the cytologist. The proper preservation of herbarium specimens can do away with this difficulty. This very simple precaution has been very largely neglected by cytologists and its neglect has not only invalidated some of their work, but has tended to make taxonomists doubtful of the value of much that has been published. In experimental science results can be tested by repeating experiments. Constantly to re-examine and re-compare specimens is the equivalent of this in ordinary taxonomy. Another difficulty is that cytological data are often based on examination of only one or two samples per species and that there are many species and genera, especially in the tropical floras, that have not been examined cytologically at all. Presumably this is a difficulty in the more general use of cytological data that will be resolved in time with the training and appointment of more cytologists. Closely connected with this is the possible danger of premature use of cytology as the main basis of new taxonomic systems. A classification based on cytology is a special and not a general classification. So long as this is remembered the taxonomist welcomes cytological classifications but he tends to resent being told that his own general system is now obsolete, is not phylogenetic,

or for some other supposed reason must be discarded.

There are, of course, many examples already available of the use of cytological data in confirming or improving a classification. Three are selected. The late Dr. O. Stapf concluded that *Spartina townsendii* was a species of hybrid origin. Huskins confirmed this by examination of the chromosomes. *S. townsendii* has 126 somatic chromosomes and the putative parents *S. alterniflora* and *S. stricta* have 70 and 56 respectively. More recently, Howard and Manton found that two chromosome numbers occur in the fertile British watercresses. *N. officinale* has $2n = 32$ and the other, which they named *N. uniseriatum*, has $2n = 64$. These cytological differences are correlated with differences in gross morphology which, as my colleague H. K. A. Shaw has shown, had been recognised long since by a few taxonomists but had been overlooked or their significance not realised in later times. It would appear that the name *N. uniseriatum* has to be replaced by the earlier valid name *N. microphyllum*. Above the species level we have an excellent illustration of the use of chromosome numbers and shapes, combined with other data, in the work of W. C. Gregory on the *Ranunculaceae*. While reserving judgment on the classification Gregory proposes, partly because some genera and a great many species of the family have not yet been examined cytologically, we acknowledge he has brought together many important facts and would seem to have fairly established that the achene type of fruit has been developed in different genera within at least two independently evolving series.

It must be confessed that plant taxonomists have not yet adequately considered the implications of the wide occurrence of polyploidy in the plant kingdom. The subject bristles with difficulties and it may well be that more data are essential before any widely applicable rules can be formulated. The taxonomist would ask cytologists not to give new names (specific or otherwise) to the polyploids they discover before thoroughly investigating the supposed synonomy of accepted species in the group under investigation. In relatively well studied floras it is probable that most polyploids have names already, although many have been 'sunk' or, at least, are not now retained as names of 'good' species.

Ecology and Plant Classification

Taxonomists, while urging on the ecologist the need for voucher specimens, have long recognised the value of ecological data and continue to urge collectors to record altitude, plant community, soil, and other facts of the habitat on their labels. They are convinced of the importance of such special schemes as that of Raunkiaer's life-forms and would be glad for the early completion of the *Biological Flora* now being published by the British Ecological Society. Obviously, it is autecology more than synecology that can provide facts of major importance in classification. One may instance the records of seed counts and germination tests in Sir Edward Salisbury's work on reproductive capacity.

Apart from genetical studies, most of what is sometimes called experimental taxonomy can equally well be termed experimental ecology. Transplant experiments of one kind or another are essential for a proper understanding of species and their make-up. Many taxonomists acknowledge the value of growing under control the plants they study. Only in this way can life-histories be described and many characters properly evaluated. It is fitting here to record the debt of botanists to horticulturists. Not only has the growing of herbs, shrubs, and trees as garden subjects led to expeditions into botanically little known parts of the world, and the discovery of many new species, but it has given the opportunity for coloured illustrations to be made on a large scale as in the nearly ten thousand plates of the *Botanical Magazine*. Moreover, and this is the immediate point, gardens and gardening make the taxonomist realise the validity of ecological data perhaps even more than field collecting. May I add that in thus referring to gardens I do not merely mean the intentional botanic garden, but include also the private garden, large or small, and even the kitchen garden and orchard need not be excluded. The taxonomist

who has never pottered about in a garden is only partially educated.

However, special problems are most likely to be solved by planned transplanting in experimental grounds. Sometimes very simple control gives useful information. Merely to grow a number of stocks under conditions as uniform as possible will often give reliable indications of their genetical similarity or dissimilarity, apart from the presence of recessive genes. Still more informative data are obtained by growing plants through several generations from seed produced under controlled conditions of pollination. Generally it is advisable to use stocks of known wild origin and to correlate experimentally obtained data with field notes. The taxonomist would like to see all the varieties named in, say, Druce's *List of British Plants* (ed. 2, Arbroath, 1928) tested by growing them side by side in an experimental ground. One may quote as examples of varieties tested in the Herbarium Experimental Ground at Kew, and found to maintain their distinctive characters and to ' come true ' from seed : *Cytisus scoparius* var. *prostratus*, *Pulicaria dysenterica* var. *hubbardii*, *Plantago coronopus* var. *mediterranea*, *Solanum dulcamara* var. *marinum* and var. *uniflorum*, and *Bryonia dioica* var. *hermaphroditica*.

More elaborate transplant experiments were those carried out at Potterne, Wiltshire, for the British Ecological Society. In these experiments various species of British plants were tested for their reactions in five different soils : sand, calcareous sand, clay, chalky clay, and Potterne soil. In addition, samples were grown at Kew. Clones were used by planting 26 ramets in every soil. The results of the experiments were published in a series of papers in the *Journal of Ecology* and to these reference can be made for many interesting details. It must suffice here to say that it is impossible to foretell, with our present knowledge, what the reaction of a species will be to the different edaphic conditions. Nearly every species reacts differently. Chemical analysis showed ' sand ' to be the poorest of the substrata used and most species gave poor growth on this. Yet *Centaurea nemoralis* made good growth, including flowering, vegetative spread, etc., on all soils and best growth on sand. Again, death rates on different soils varied greatly from soil to soil. Some species, as *Silene maritima*, showed little size and few structural differences from soil to soil, while others, such as *Plantago major*, quickly changed in these characters on some soils. In the last-named species, changes were obtained that placed the ramets of one clone into different so-called subspecies of a recent classification. There is no doubt that such experiments, extended and modified in many directions, can throw much light on the numerous problems of variation which so often puzzle the taxonomist.

Very extensive transplant experiments were initiated by Hall and Clements in America and have been extended by Clausen, Keck, and Hiesey. Many of you will be familiar with the series of books and papers on their work published by the Carnegie Institution. One acknowledges the deepest interest in the results with a feeling of envy at the facilities available in California for carrying out the experiments. Most of the species used in these researches are not natives of Europe and there may be some difficulty for botanists not familiar with the Californian flora fully to appreciate all the conclusions reached. The attention of British botanists should, however, be called to the investigation of the ' Environmental responses of climatic races of Achillea,' and the hope is expressed that parallel investigations may be undertaken in this country on the British and European yarrows placed under or closely associated with *Achillea millefolium*.

Genetics and Plant Classification

The contribution of genetics to classification has hitherto been more indirect than direct. In the first place, to study a group by genetical experiment necessitates examining, one might say living with, many individuals throughout their life-histories. Only thus can characters be properly appreciated and fully evaluated taxonomically. The geneticist has to distinguish fluctuating modifications from genetically fixed differences or, if there be overlapping, to recognise and as far as possible analyse the mixture. The taxonomist frequently has the same

problem and must welcome data, relevant to its solution, supplied by the geneticist. Genetical research concentrated on one or a small group of species is perhaps the best way of understanding the very great range of genetical variation that often occurs in a single taxonomic species. I can only say that it has been an education to me to have carried out extensive experiments with my colleague Marsden-Jones, in particular on the bladder campions, knapweeds, buttercups, and saxifrages. In the bladder campions, for example, variations in characters, most of which have been shown to have a genetical basis, occur in nearly every organ : stems, leaves, inflorescences, calyces, petals, stamens, gynoecium, fruits, and seeds. They are also found at every stage of development from the seedling to the fruiting adult. These variations occur in unnumbered combinations and the number of 'varieties' must reach astronomical figures, in spite of some obstacles to some character combinations. These variations must be taken into account by the taxonomist, not, of course, by the giving of millions of varietal names but in the drawing up of his descriptions of species (and other taxa) and in deciding what intraspecific variants specially need taxonomic recognition.

Another great help to taxonomy provided by genetical research is the proof or disproof of the hybrid nature of what some taxonomists have accepted as species. Thus, in the knapweeds of the *Jacea* subgenus of the genus *Centaurea*, we have proved by analysis and synthesis that a majority of the so-called species of the British flora are hybrids which segregate on selfing, or crossing two stocks together, for there is a high degree of self-sterility in most stocks, and which can be made by crossing true-breeding parents of *C. jacea* and *C. nigra* (or a close relative) together. The problem of hybrid swarms frequently confronts the taxonomist and though field studies may strongly suggest the composition of such populations, genetical experiments are essential for proof and for establishing the constitution and status of individuals showing this or that combination of characters. There is a wide field of research open here. Preliminary taxonomic investigations have suggested that hybridisation between a limited number of species may be the explanation of the diversity of characters and character combinations in various tropical groups. One example must suffice here. Recently, my colleague Milne-Redhead and I had to bring order out of chaos in the East African material of *Clematis*. After much labour we produced a tentative scheme in which we postulated gene-flow between several species. Only thus could we avoid throwing over the species concept or so modifying it as to make it scarcely recognisable.

Plant taxonomists of wide experience generally do not accept, in theory or in practice, the extreme view that the one (or major) criterion of species is the occurrence of a sterility barrier between every taxon to be accepted as a distinct species. To do this would lead, in many groups, to a classification that was a special and not a general one. As such it would not be of the widest use, either because it would unite taxa which for most purposes have to be distinguished, or because the nomenclature would become too cumbersome for convenience. There are also other difficulties such as the occurrence of all degrees of sterility and fertility, of self-sterility, of bridging species, subspecies, or varieties, the usual lack of data, and the difficulty, if not impossibility, of proving complete inter-group sterility. On the other hand, there is no doubt that sterility barriers are often of very great importance in maintaining species as distinct entities. The taxonomist has frequently to consider the converse of this. If specific distinction is due to some other cause than cross-sterility, and the cause be removed, then two species may either form hybrid swarms or completely amalgamate to form one more or less highly variable species. Properly to test sterility-fertility characters is a task for cytogeneticists and the results obtained must be considered by taxonomists for their final evaluation and incorporation in a classification.

Morphogenesis and organogenesis in relation to plant classification

Since classification in the seed-bearing plants is, and must continue to be, largely based on gross structure, any researches

into the details of the morphology, development, and origin of organs must be of taxonomic importance. The terms morphogenesis and organogenesis have been used in two senses : the development of an organ in the course of ontogeny and the development of an organ from a primitive state in the course of evolution. It might be useful to use morphogenesis for the former and organogensis for the latter. Only in so far as ontogeny recapitulates phylogeny will the processes coincide. From the standpoint of classification the two phenomena have one feature in common : each is concerned with a character-series changing with time.

In the angiosperms, ontogeny has been relatively little used in classification except to support some phylogenetic hypothesis and in so far as this may affect a proposed classification. There are, it is true, many scattered observations on the structure of seedlings and juvenile forms, but I know of only one flora in which an attempt has been made to aid determination of flowering plants at any stage of vegetative growth. Unfortunately, that flora, a quite recent one by Duvdevani and Osherov, is in modern Hebrew (see *Kew Bull.* 1948 : 45–46) and deals only with 600 species of Palestinian plants. Even this flora does not use ontogenetic characters for improving classification as such. There would appear to be a wide field for experiment here. The suggestion is put forward that the British flora might be investigated from this standpoint, as a start, by collecting or working out the ontogenetic data of the species one by one and then attempting to use the data in classification. The results might be surprising for the theory of classification, they would certainly be useful to ecologists and many others who have to deal with plants at all stages of their life-histories.

The study of what Zimmermann has termed ' the phylogeny of single characters ' has received a good deal of attention in recent years. One may instance the various theories as to the nature of the stamen and the carpel, attempts to distinguish primitive and advanced types of pollen-grain, male genetophyte, embryo-sac, etc., and the opposed results of ' phylogenetic ' analyses of inflorescences. The systematist is always interested, more or less, in such studies. There is, however, often such divergence of opinion that the working taxonomist, who, even if he be ' left ' in politics, is nearly always cautiously ' right ' in his own branch of botany, usually decides to wait before incorporating any new hypothesis in a modified classification. It must, also, always be remembered that ' character phylogeny ' can, at best, only give a special classification which frequently will be different from other special classifications.

Plant geography and plant classification

The geographical range of taxa, particularly at and below the species level, has always to be considered in classification. Probably none but a taxonomist working in a large herbarium, having to handle many collections and to work with especially difficult groups, can properly appreciate the importance of knowing distribution as an aid to determination on the one hand and as a help in settling status on the other. It is, I think, true to say that taxonomists are themselves paying more and more attention to the geographical distribution of their units. This year both the Systematics Association and the Botanical Society of the British Isles have held symposia on botanical mapping, the former in collaboration with the Royal Geographical Society in connection especially with projections, scales, symbols, and other matters in connection with the graphic illustration of ranges over wide areas of the earth's surface.

The facts of distribution accumulated by the taxonomist naturally lead him to investigate causes, and many taxonomists find, as did Sir Joseph Hooker, a century ago, a major interest in attempting to trace the origin and history of floras. In phytogeographical research the student has the widest possible field since, sooner or later, he has to take into consideration data from almost every field of knowledge. Sound phytogeographical research is impossible without sound taxonomy. The converse is also often true and is one of the reasons why taxonomists frequently dislike having to deal with plants of unknown wild origin, as, for example, garden plants whose history has not been recorded. The modern taxonomist always pays careful

attention to differences in, or abnormalities of, range. It is of great service when latitude and longitude are recorded on labels and given when specimens are cited.

Palaeobotany and plant classification

It has frequently been pointed out that the existing flora of the world is only a cross-section of its evolutionary history. Without the concrete evidence of fossils in known stratigraphical sequence any reconstruction of phylogeny has to be based on assumptions which, to be valid, must be proved by evidence independent of that used in the reconstruction.

For the angiosperms, fossil evidence relevant to phylogenetic studies is extremely meagre. Even apart from phylogeny, palaeobotany has had little to give as an aid to classification of the flowering plants. For the *Pteridophyta* and *Gymnospermae* more palaeontological data are available and, in the former particularly, have influenced modern classification very considerably.

The attention of botanists may be called to a recently delivered lecture by A. Tindell Hopwood in which the impact of palaeontology on classification was carefully considered. The lecturer showed that, while a knowledge of the fossil history of a group may solve some problems for the taxonomist, it raises others. He pointed out that even for groups in which relatively complete evidence of evolution in time is now available, the classifications so far proposed are either classifications of a series of cross sections or a compromise between such and a classification based truly on phylogeny.

Statistics and plant classification

Many modern taxonomists are recognising, more and more clearly, not only that species must be regarded as populations but also that, for their proper study, there must be adequate population sampling with all the safeguards demanded by biometricians. Frequently, it is unsafe to accept herbarium collections as fulfilling the rules of random sampling. I recall that soon after I began to work at Kew, over forty years ago, I had to consult the covers of *Poa annua*. There were many specimens of giant forms, of dwarf forms, of variants with exceptionally loose or very contracted inflorescences, and so on, but very few indeed that one could regard as ' typical,' as approximating in the sum of characters to the majority of specimens in most colonies of this commonest of all our garden weeds. The taxonomist would, then, ask for suitable material for statistical studies with a taxonomic aim. For such he must either depend on trained collectors or, better still, be given reasonable facilities for field studies himself. Time and again in monographic studies or in investigating groups as part of a flora I have felt the need for series of good specimens that could be regarded as in every way a ' fair ' sample. Pertinent examples are : *Fritillaria*, *Fraxinus*, *Thymus*, and many species I have had to ' work out ' for the *Botanical Magazine* in recent years.

There are two aspects of statistical studies that must be briefly touched upon here. First, how far the taxonomist should be expected to carry statistical analysis when he has the material available. There is no doubt that measurements should be given in preference to such relative terms as large or small, long or short, and so on. It may be suggested that wider use should be made of maximum, minimum, mean, standard deviation, correlation, contingency and goodness of fit. Secondly, the taxonomist has often to deal with data in a way for which no adequate strictly statistical method is available. Here there is yet another wide field for students with adequate training, experience, and ingenuity. In particular, two lines of practice appear in need of extension : devices for describing in statistical terms characters involving asymmetry and other kinds of irregularities and contrivances for summating association of characters above the correlation of only two characters per item. Anderson, Melville, and others have made useful preliminary attempts to find solutions for more or less special problems coming under such headings.

Generally, taxonomists are not highly trained and skilled mathematicians, and it would appear desirable that biometricians should be invited to turn their attention to the needs of those of us who require their help in systematics. We shall have to explain our problems frankly

and ask for formulae and graphic methods which are not only statistically sound but are as simple in application as possible. It is probable that there are limits to the use of statistics in general taxonomic botany and it is at about the species level that statistical methods are most useful. The taxonomist requires statistics for the sake of taxonomy, not statistics for the sake of statistics.

Phylogeny

We have concluded a rapid survey of the data now being incorporated in taxonomy. Much more could have been added but perhaps enough has been said in support of the contentions that plant taxonomy is a living, growing, changing subject, and that taxonomists are not hopelessly out-of-date describers of species old and new, who for recreation spend their spare time in nomenclatural quarrels and quibbles.

However, one matter that has been mentioned only incidentally must not be deliberately side-tracked. Phylogeny, like ' Mesopotamia ' in the first world war, is somewhat of a ' blessed word.' It has a pleasing comforting sound which no doubt does something to lessen the high-brow austerity of the word taxonomy. Phylogenetic speculation can undoubtedly be a great stimulus to research and for this reason can be fully justified. Judiciously used it can also be made to link together otherwise isolated facts and may thus become a convenient mnemonic device. The danger is that phylogenetic speculations tend to be presented as facts and even to become accepted as such, at least by the speculator. In a paper published in *The Botanical Review*, 1942, I went rather fully into the connections, as I saw them, between taxonomy and phylogeny and here only wish to emphasise one or two points that seem of considerable importance. To describe a classification as based on phylogeny surely implies that the phylogeny is known previous to the making of the classification in question. It seems to me that such knowledge can at present only be obtained directly in two ways : either by witnessing evolution or by studying its results as seen in series of fossils in the rocks. As to the former, it is only at about the species level that we thus have, or can have, evidence of how taxa have originated and developed phylogenetically. Synthesis and analysis of polyploids, the appearance of mutations, and the effects of hybridisation have thrown much light on the origins of species and intraspecific variation but very little on the phylogeny of the groups of higher taxonomic rank. For relevant fossil evidence, in the angiosperms, we are still waiting. Personally, I am forced to conclude that if we are to obtain sound evidence for constructing a phylogeny of the flowering plants, it will come from palaeobotany brought into line with what is known of the cytogenetics of existing plants. Whether such phylogeny will serve as a basis for a practical classification is doubtful, since it is inconceivable that a phylogenetic scheme can be linear, and whatever its type it would be a special classification.

Any known phylogenetic facts must of course be considered in preparing a general classification and, as far as possible, they must be included in the criteria used. Moreover, rational phylogenetic speculation may be of considerable help in suggesting improvements in a classification, with the strong proviso that it is acknowledged as speculation and is not confused with direct evidence.

There is no need here to discuss the various classifications that have claimed to be phylogenetic or based on phylogeny: those of Hallier, Bessey, Hutchinson, and others are well known and no doubt more will be forthcoming in the future.

There are, however, three papers, published recently by two authors, that have considerable bearing on some of our taxonomic problems in relation to phylogeny and which it seems desirable to appreciate by criticisms.

Sporne, in two interesting papers, has statistically analysed twelve vegetative or floral characters, or better character pairs, in the families of dicotyledons. Some of these characters tend to be correlated together and, it is claimed, form two groups termed primitive and advanced, respectively. The former were established as such because certain of them occur more frequently among the

41 families recorded by Reid and Chandler for the London Clay flora than among the families of the world flora. Additional supporting evidence was obtained by comparing 47 families determined by fossilised wood with such modern families as have woody members. The analysis led to arranging the families of dicotyledons in a sequence of groups from the most primitive to the most advanced. The family *Flacourtiaceae* is placed as the most primitive and the three families *Dipsacaceae*, *Martyniaceae*, and *Phrymaceae* have equal value as the most advanced families. The ancestral dicotyledon is pictured as a tree or shrub with alternate, glandular, stipulate leaves with unisexual, actinomorphic, polypetalous flowers, with many stamens and many carpels, and with seeds with two vascularised integuments outside which was an aril.

It must be emphasised that Sporne has made a laudable attempt to determine primitiveness objectively, and that, in these papers, he is not otherwise concerned with the phylogeny of the families. One criticism is that the author's scoring is inadequate, probably because it has been taken from condensed published accounts of family characters. Certainly, a considerable number of his plus or minus scorings should be replaced by plus and minus. It is very difficult to pick up exceptions unless one knows a family well by actual experience. Only a few examples can be given here. In the *Caryophyllaceae* there are a few shrubs, a considerable number of species with unisexual flowers, and some, like our common bladder campion (*Silene cucubalus*) with zygomorphic flowers. In *Salicaceae*, hermaphrodite flowers have been recorded for both *Populus* and *Salix*. It may be that the inclusion of all ' exceptions ' or ' rarities ' would not greatly alter the advancement index values and the final evaluation of primitiveness or advancement, but the taxonomist knows they are much more numerous than is recorded in text-books.

Another possible source of error in scoring is that, unless one is personally very familiar with the structures, terms rather than characters are scored. In other words, are all ' stipules ' or ' arils' or all examples of the tree and shrub habit, and so on, genetically and phylogenetically homologous ?

Dr. Sporne is fully aware that the inclusion in his scheme of other characters might alter somewhat the advancement index values of some families. ' Primitive ' and ' advanced ' are relative terms and it seems a little risky to picture an ancestral dicotyledon from the correlation of characters of existing families, even if such characters are compared with those shown by families existing in Eocene times. Dicotyledons existed long before the London Clay was formed—possibly they arose as far back as the Triassic period. One wonders too, how far Sporne's methods are statistically sound if the *Angiospermae* be polyphyletic.

Corner, on the Durian theory or the origin of the modern tree, presents for criticism a stimulating paper. Basing his ideas on the structure of the fruits and seeds of the durian (*Durio zibethinus*, *Bombacaceae*), Corner concludes that the primitive angiosperm fruit must have been a red fleshy follicle, probably spiny, with large black seeds hanging on persistent funicles and covered with a red aril. By considering various correlations and introducing several, perhaps rather debatable, premises, it is then argued that the primitive angiosperm must have been a mesophytic, tropical, Cycad-like, monocaulous tree with large pinnate leaves and peltate scales, probably monocarpic, and producing a terminal cluster of large follicles with arillate seeds. The modern tree was derived by reduction in size and complexity of the branches, leaves, flowers, and fruits, and the evolution of axillary inflorescences. Corner raises many questions in the mind of a careful reader of his paper—which is all to the good—though some of his reasoning is difficult to follow. Apparently the basis for accepting arillate seeds as primitive in the flowering plants is in the scattered occurrence of such structures amongst angiospermous families now existing. It is stated that ' There is no means of evolving an aril *de novo*,' and yet (p. 373) a list is given of twelve families, usually regarded as widely scattered in any system of classification, with a ' rudimentary aril.' Apparently, even in existing families, rudiments of an aril can start at

more or less a dozen points. It is, however, possible that there is here confusion of terms and that the author intended to write ' vestigial ' and not ' rudimentary.' Nevertheless, whether small arils be ' vestigial ' or ' rudimentary ' they exist in large numbers of species of various families and it is surely risky to say that small arils are useless, even in undetached seeds. They certainly have a value in dispersal in some detached seeds. Birds, bats, and arboreal mammals do not comprise the sum total of the fauna of tropical forests. One wonders if all the structures, that have been called ' arils ' are strictly homologous. Such a statement as ' what is abundant now cannot possibly be primitive ' (p. 377), or ' no herb could produce a durian, a nutmeg, or an Annonaceous fruit, far less the Leguminous cluster of arillate pods ' (p. 390), or ' Monocotyledons which may be taken as representing the proto-Dicotyledons, just as Conifers and Gnetales are modern representatives of the proto-gymnosperms ' (p. 410), or the generalisation ' that the seeming oddities of which I have written form a demonstrative whole should prove themselves that, without tropical orientation, biology is lost ' (p. 369), leave one wondering either what is the evidence or what is meant. The main criticism, however, must be to request more evidence that scattered characters, which for all organs together result in a taxonomic reticulation, are primitive. Such a line of argument could be applied to a great many other characters than arillate seeds : *e.g.*, rhizomes, stipules, tendrils, ruminate endosperm. This is, indeed, exemplified by Corner himself in postulating primitive form in the somewhat strange mixture of examples he gives (p. 396).

Corner has certainly raised a great many fundamental questions, as well as some less basic ones, and these require much more thought and elaboration and detailed criticism than they have yet received.

The possibility of new methods of classification based on new principles

Nothing has yet appeared to replace, on any large scale, the principles of orthodox taxonomic schemes in the seed-bearing plants. Families, genera, species, and varieties, are given Latin names and arranged in a hierarchical system which still shows few signs of decline and fall.

Nevertheless, there are many groups where the species concept has to be used in a very arbitrary manner, and some in which the genus concept appears in practice as an artificial device retained in order to avoid upsetting a widely used nomenclature. It may be that these defects will be put right through the extension of methods we have already discussed, without a total reconstruction of theory and practice. It is, however, worth while reminding you of some suggestions that have been made by botanists.

Hayata, in a number of papers, developed what he termed a ' dynamic system.' His writings are not easy to understand in all parts, but he was undoubtedly impressed by the wide occurrence of radiating affinities resulting in a network system of classification. He recognises the principle that a system should be subject to change according to the point of view of the taxa taken at any time. The species, genera, and families can be moved about according to the characters under consideration. There is no doubt that the facts put forward by Hayata, and many others of similar nature, require the careful attention of taxonomists. Not only do they still await explanation in terms of phylogeny, but the practical use of a " loose-leaf system of classification,' if one may coin a phrase, with plenty of cross-references, needs investigation. There is a strong possibility that an understanding of what is behind reticulation of characters, at the different levels of families, genera and species, may lead to important discoveries of still unknown methods and modes of evolution. Reticulation of characters is in some ways the opposite of correlation. What I have termed true and false reticulation will have to be distinguished. Graphic, and perhaps later statistical, methods of treating the facts will have to be devised. At any rate, there is here a fascinating field of study along the lines of the new systematics.

For analysing populations and families

of the bladder campions, Marsden-Jones and Turrill prepared a scheme of tabulating characters in shorthand symbols by means of which the character composition of any individual can be compared with that of any other individual. Similar schemes have since been applied successfully in other genera than *Silene*. The advantages of the method are several: it is elastic since the number of characters used can be increased or decreased, scoring follows the same pattern for every individual, and this both increases the rate of scoring and aids precision, long descriptions are replaced by short formulae, and summation vertically and comparison horizontally can be easy and speedy.

Rabel has outlined a similar scheme with a much wider scope which, if perfected, might be applicable not only to individuals of a species, but to all taxonomic categories of vascular plants. It could be extended to all plants, but would probably then be very cumbersome. My own view is that Dr. Rabel has little idea of the complexity of structure to be found in the plant kingdom and that for any general scheme of the kind she suggests the number of figure symbols would be so great that the scheme would not be easy to work. However, it should be tested widely with certain modifications.

It has to be confessed that letter and figure symbols are uninteresting and monotonous compared with names. This is for well-known psychological reasons which need not be discussed here but it has two practical implications. First, they are less easy to remember, while secondly, it is less easy to spot mistakes in scoring or proof-reading. It is probable that both terms and plant names will long be used in verbal form.

There is one matter which may be mentioned in this place since it concerns equally character symbols and terms. In both genetical and field population studies it is imperative to define with a determined degree of precision the class limits used in scoring any character. In taxonomic descriptions the definitions of terms are far too frequently vague or are not fixed by international usage. There is great need for an authoritative glossary of botanical terms but, I would suggest, it should be prepared by working taxonomists in collaboration with philosophers who have had a good training in logic and its practical application to definitions of terms—if such philosophers exist.

Training in Taxonomy

At the present time there is a dearth of trained taxonomists. It is true that the demand for taxonomic specialists for filling botanical posts is not so great as it is for applied botanists. Nevertheless, it is increasing and is likely slowly, but steadily, to increase. Taxonomists have, however, what may be a more serious criticism to make of schools and universities in the matter of training in taxonomy. The present Director of the Royal Botanic Gardens, Kew, Sir Edward Salisbury, in his Presidential address to this section at the Nottingham meeting of the British Association in 1937, lucidly stressed the need for a wide training to be given to those taking a science degree. The principles he emphasised apply not only in the relation of botany to other branches of science, but to taxonomy in relation to other divisions of botany. Specialisation there must be in due time or the student becomes a jack of all trades and master of none. Yet, it is surely undesirable to give an honours degree in botany to students who have very little, if any, acquaintance with either the principles or practices of plant systematics. Up to quite recently, in most universities in this country, taxonomy has been, at best, relegated to the position of Cinderella before the fairy godmother appeared on the scene. If the statements made earlier in this address cannot be refuted, such a position is bound to be harmful to our science as a whole. Classification is basic to every science, and taxonomy to every branch of botany. Heaven forbid that every student of botany should be trained to become a taxonomic specialist, but every botanist worth the name should have a good working acquaintance with the general philosophical principles of classification, with their application to plant life, and with the materials used by and the special problems facing the taxonomist.

Fortunately, there are signs that taxonomy is becoming reinstated in botany as

a subject taught in our universities. We taxonomists appreciate the action of the University Grants Committee in making possible the creation of teaching posts in taxonomy in several university departments. It is particularly appropriate to express this appreciation here since the first lectureship in botanical taxonomy under this scheme was filled last year in the University of Birmingham. Perhaps we may even look forward to the creation, in the not too distant future, of chairs of botanical taxonomy.

It has to be confessed that taxonomy is not the easiest of subjects to teach. The best approach, in my opinion, is a double one : to teach the student to teach himself the practice of taxonomy and to guide him to an understanding of the fundamental principles of classification.

As far as possible, this double approach should be unified : principles become dogmas unless constantly applied to and tested by facts ; merely to collect facts is to make bricks without building. Lectures in taxonomy should be reduced to a minimum ; field and laboratory studies should be encouraged rather than reading, and should be maintained throughout the year, not just squeezed into the summer term. Above all, in teaching taxonomy, as in taxonomic research, the newer methods should be given prominence. If this be done and students complain that taxonomy is dull and of little importance, there is something genetically and incurably wrong with the students.

Conclusions

In bare outline an attempt has been made to cover the wide field of plant taxonomy, with special attention to its modern trends in the seed-bearing plants. My aim has been to draw the attention of students and teachers to its potentialities. Plant systematics is not a dead subject limited to the study of ' bits of hay ' stuck on herbarium sheets and pigeonholed in standardised cupboards. Without classification no research is possible. Even the plant physiologist has to say he is experimenting with sunflowers. Radiating from his central task of sorting the wealth of the plant world into distinguishable classes, the taxonomist finds paths leading directly to the study of the composition, origin, and development of floras, to speculation on the origin and changing make-up of taxonomic units, to considerations of heredity, hybridisation, and morphogenesis, and finally to the how, why, and wherefore of all that lives or has lived. To many this may sound exuberant exaggeration. Those who have taken part in our debates at the Systematics Association, or in the Director's Office or staff room at Kew, will consider it an understatement. Taxonomy is coming back into its own along modern lines, and younger students need have no fear that if they become taxonomists they will be specialising in an out-of-date subject with only an artificial interest. Nowadays, we taxonomists seek for data from ecologists, geneticists, biochemists, physiologists, horticulturists and all the other exponents of this or that branch of our science. We go outside the ranks of botanists and request help from zoologists, geologists, geographers, and many other specialists. No shame at our importunity, no false pride at our borrowing, and no hesitation in proclaiming the importance of our work prevent us from obtaining and using facts gleaned from many fields in order to make our classification as general as possible. The narrow taxonomist is dying out and will soon be known only by his fossilised records. Very probably he has always been much rarer than many biologists think, especially if they are not well acquainted with the history of taxonomy.

Nevertheless, taxonomists have their special problems, their original methods, and, one must add, their own peculiarities of outlook and criticism. They would form interesting material for psychological investigation. The satisfactory taxonomist is born, not made, at least in the sense that he must have a certain number of innate qualities. He must have a methodical mind with a liking for bringing order out of chaos. He should have a keen aesthetic perception of the beauty of shape, colour, and the relationship of parts. It may sound strange to include qualities usually considered the peculiar attributes of the artist, as necessary to the make-up of the satisfactory taxonomist.

Yet, if you consider taxonomic practices it must become clear that quickened appreciation of curves and other outlines and of balance of organs aids comparison at every stage. There is no doubt that many taxonomists are born collectors. This may be interpreted as their being endowed with an undue share of the acquisitive instinct. This needs to be kept under control but, as expressed in herbaria, is at least relatively harmless and may then be regarded as a sublimation of such developments of the acquisitive instinct as shown by many historical figures, from King Solomon to Hitler and Göring. The tremendous symbolic and possessional value ascribed to names from very ancient times, perhaps, explains in part some of the aberrations of nomenclaturists.

Finally, may I express the hope that what I have said here may contribute to the wider understanding of what are the aims and methods of modern plant taxonomy. We taxonomists desire to make our classifications more and more useful and can only do this through co-operation with all our colleagues. We work to serve and ask only for such recognition as we fairly deserve.

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EDUCATION AND POWER

Address by

MR. W. F. OAKESHOTT

PRESIDENT OF SECTION L

EVERBODY knows that there is something unusual about the age in which we live. All other ages have always regarded themselves as ages of transition. Ours seems likely to be an age of explosion. That learned man, Sir Patrick Geddes, used to call the early part of the industrial revolution (the period I mean, when steam power was being developed, when the building of factories and slums was taking place) the Palæotechnic age. It was followed by the age of the dynamo, of the motor car, the aeroplane, and the wireless; which is the neotechnic age. But ours is one move further. We have stepped into the Apocalyptotechnic age, one symptom of which seems to be, as you will soon experience, that its educational platitudes have a preternatural monstrosity. Yet there are few more important themes I suppose, than that of Education and Power at such a time. You all know those wireless sets with what is called by the manufacturers finger-tip control. You touch a button, and a sort of Aladdin's cave of sound is opened to you in an instant. With the lightest movement you switch off the Professor in the Third Programme, and suddenly are in the charmed company of the Ovaltineys (or whatever they are called) from Radio Luxembourg. What more notable sign of progress, we may be permitted to ask incidentally, could there be? It is only a matter of a few short years, for the like movement to bring into your drawing-room, for you to see as they happen, the most insistent advertisements of cosmetics in the television programmes from Chicago. Now this might mean that we are rushing very rapidly towards the millennium—though personally I incline, at any rate in less optimistic moments, to scepticism on that account. What it evidently must mean is that we are rushing somewhere. And in so many other instances nowadays, the invention is produced in the first place because there is an interesting technical problem to be solved rather than because there is a need to be filled. Its solution brings in its wake a whole set of social problems. In the social field we act again and again in the spirit of the man who introduced the rabbit into Australia. That enterprising gentleman doubtless had intentions which were wholly good. How delightful it would be for the baby to watch a few bunnies gambolling at the foot of the garden! The great inventors of modern times have been, equally, men of excellent intentions. Yet those responsible for the cultivation of souls have found the ground infested with some new brand of rabbit often enough during the last century—pests which have looked like creating a desolation and making good husbandry impossible, and there is no sign of the plagues being got under control. Balance in society, or education, means no more to the technicians—nor to organised society—than the balance of nature meant to that agricultural pioneer. How are such changes to be directed?

This matter of the unexpected power generated by technical innovation is only one side of the problem. One of the things which differentiate the phases of history, and the various societies which form it, is the aim which an ordinary ambitious citizen proposes to himself, as that which he wants to achieve. There has been one period (but only one) when the scholar was reckoned by almost all sections of the community as its most distinguished figure. The melancholy fact must be conceded that never was there a period of greater moral chaos, and of less real purpose. There have certainly been times

when money has been the aim. Uncle Timothy Forsyte was determined, if you remember, that one at least of the Forsytes should at long last enjoy a really respectable fortune. Old Gradman, the family lawyer, on hearing the will read, which left the greater part of Timothy's property and capital to accumulate for 'that lineal descendant of my father . . . who after the death of all the first generation of his descendants should last attain the age of twenty-one . . .' tried to calculate what it might be worth. 'And Mr. Timothy,' he said, 'worth a hundred and fifty thousand pound nett. if he's worth a penny. Compound interest at five per cent. doubles you in fourteen years. In fourteen years three hundred thousand—six hundred thousand in twenty-eight—twelve hundred thousand in forty-two, twenty-four hundred thousand in fifty-six—four million eight hundred thousand in seventy—nine million six hundred thousand in eighty-four—Why, in a hundred years it'll be twenty million! And we shan't live to see it! It *is* a will!'

But to the man who proposes for himself the aim of making money the Chancellor of the Exchequer has now pronounced with devastating finality his *sic vos non vobis* . . . And let us make no mistake—in this he is only the mouthpiece of his age. Nowhere, not even in America, is it likely that the vast fortunes of the nineteenth century will again be accumulated. The aim of the ambitious man in the eighteenth century was not to amass a fortune, but was something more civilised; to use his money to show his taste; to build a great classical mansion set in a splendid landscape designed for it; to fill a shelf in his library with poems and treatises dedicated to himself by recipients of his bounty. In other societies, in the oriental societies for example, different aims again have been held. But if it were said that the most usual aim of ambition in our present society is power, the statement would not be far wrong. I remember working as a young man in an office—and it was actually a very good office—with someone whose job it was to deal with the applications made by the headmasters of elementary schools for supplies for their schools: supplies of books and equipment of every kind. What this man liked in his job was its power, the power it gave him of doing harm to better men than he was; the power it gave him of seeing that his favourites were well served, and those who showed too much independence and too little recognition of his position, badly served. This example is quoted to show that what is in my mind is not simply the power of the politician; though when societies have recovered their sanity, they will surely regard it as one of the most extraordinary features of the present time that such fantastic powers were allowed a Hitler or a Stalin. And if our own politicians are not named in this context (with some in other key positions in our own society) it is from reluctance to allow the suggestion of party political implications, about what seems to be a feature not of any one party, but of this time. A hundred years ago the incomes of the rich were growing with extraordinary speed, and the incomes of the poor were shrinking. We woke up in three generations or so, to find the situation revealed by the chapters on income in Sir Alexander Carr-Saunders' book: *The Social Structure of England and Wales*—an unprecedented spread of income between the very rich and the very poor. During the last twenty years the process has been put into reverse with a speed that is in itself well-nigh catastrophic. The question is whether now something very similar is happening, almost unnoticed, in the sphere of power; something which is the more dangerous because of the potentialities of the tools and equipment which the man in a position of power has at his disposal.

It is easy to exaggerate in making generalisations, and I am particularly anxious not to exaggerate here. What is happening in England is certainly not yet comparable with what the evidence seems to suggest for Russia. Even there, the trouble is that information is so wretchedly meagre. Many others must have tried in the last four years (as I have tried) to get permission to go to Russia in the hopes of collecting at least some impressions of what was happening in this important respect. Most of them will have been put off in exactly the same way as I have been put off: by the suggestion that, if one could make oneself *persona* sufficiently

grata to the Soviet authorities, something could doubtless be done about it. It may be that we slander the regime by suggesting that its guiding ambition is the desire for personal power. Some nineteenth-century philosophers held that the resultant of the economic struggles between all particular individuals (each striving for his own personal economic advantage) was the general benefit of the community. So this striving of each individual for power might be claimed, perhaps, to result in the general advancement of the community. Once again, I am sceptical. The result seems more likely to be considerable periods of deceptive calm, followed by cataclysmic upheavals when supreme power necessarily passes out of the hands of an individual or group. A feature of volcanoes is that the word progress cannot plausibly be applied to their behaviour. But, as has been said already, not only is there nothing in this country as yet in any way comparable to the Russian situation, but also it is not so much in the political sphere as in the administrative, that danger signals are to be observed. Am I right, I wonder, that the new form that ambition takes is ambition for power? Ambition to command a file of secretaries, a battery of dictaphones and typewriters, and a regiment of telephones—that instrument designed to bring our neighbour within our home, but used in fact to keep him at rather more than arm's length? Ambition to make decisions that will affect ten million men? Ambition to carry out a vast plan because it is a vast plan, not because it is a *good* plan? That is, at least how the situation strikes me—and once again, it must be emphasised that this is not a speech from a party platform. I am simply trying to discern the face of the sky. There is a cloud in it the size of a man's hand. Perhaps the sky will be clear in a matter of moments. I am doubtful. But others are far better placed to discern the face of the sky than I am.

Of these two problems, the problem of technical power and the problem of personal power, may I take the second first—it being in many ways the simpler to understand, though not perhaps easier to solve? It is essentially a problem of leadership. Not long ago I was at a gathering of schoolmasters. The older members of the society, which has a long history, were reminiscing, and their theme was, 'there were giants in those days.' One of them said incidentally that to be a good headmaster, a man must have a considerable number of qualities that are distinctly unpleasant. Whether his diagnosis of the cause of the lack of good leadership is correct, we need not now consider. My purpose is to relate this question of leadership and power to the principles set out in two of the world's great books. One of them was written during the dark ages, by St. Benedict, the architect of monasticism in western Europe. His *Rule* for the conduct of a monastery, though so many communities so often fell short of its precept, was one of the most powerful influences on the history of the six centuries before 1300. The other is Machiavelli's *Prince*, almost equally influential during the last four modern centuries. In these books the two figures, the Christian leader, and the unscrupulous prince, the ideal 'Father in God,' and the successful dictator, can be compared, and it is the features of that comparison that I propose to try to underline. Briefly, it consists in this. The dictator accepts human nature as a poor thing. Man is a creature easily duped. If you don't dupe him for your own advantage, someone else will. Dupe him and good luck to you.

'For it may be truly affirmed of mankind in general that they are ungrateful, fickle, timid, dissembling and self-interested; so long as you can serve them, they are entirely devoted to you; their wealth, their blood, their lives, even their children are at your disposal when you have no occasion for them; but in the day of need, they turn their backs on you.'

The greatness of Machiavelli's account of the state lies not only in its brilliance, but in its shrewdness. As an account of the facts of human nature, it may be one-sided, but it goes very deep. In our own day it has been elaborated with more ruthless cynicism. There is a streak of cruelty and hatred in all of us; only give a man regular opportunities to exercise these vices, it was said, and he will be your unquestioning follower. Give him an object to hate and to bully. He will then

bow down and worship you, in gratitude for your gift. And this is not merely a matter of theory, but of tested practice—and tested by practice it is found to have an appallingly large measure of truth about it. And so, in this account, leadership means knowing the facts about human nature, realising how to turn them to your advantage, not being hoodwinked by people who pretend to be generous or affectionate or disinterested. That is just one of the moves in the game. The implication of this view is that the one thing to be desired and sought is power. If we can use the weakness of other people in the struggle for power, let us use them fully; for the weaknesses of men are their only qualities on which we can invariably depend.

The plausibility of such observations is a matter of common experience. We know for example, the importance to a military unit of the two or three men responsible for it. They are so important because people are on the whole easily led, because a man in authority, if unscrupulous enough to appeal to the weaker nature of his associates, can easily score cheap success.

The second of the books I mentioned, St. Benedict's *Rule*, is not concerned with cheap success. Nor is it concerned (as many of the books about Christian societies have been) simply with waffle. I sometimes think that someone ought to write a thesis about waffle in the literature of the Church. He would find that the great period of waffle was not the medieval period—that age of incisive and logical thinking—but the present. He might perhaps profitably devote an appendix to waffle in the ethical pronouncements of the scientific humanists. The *Rule* of St. Benedict is something very different. It has more hard-headed wisdom in it than almost any other book I know. But whereas Machiavelli, writing at the moment of the triumph of humanism, draws a picture of human nature unrelieved by the light of a single decent quality, Benedict, writing in an age when society was tottering to destruction, writes of an institution where the discipline of reason is the controlling influence, where the man in authority is as much subject to that discipline, in his own sphere, as is the youngest novice; where he knows that, so far from using the base qualities of his associates for his own aggrandisement, for every fault in them the man who rules over them is answerable to a higher authority; that his preferment of one against another must be determined not by favour but by reason only; that his work is a cure of souls, that every individual *is* an individual; that the test by which an institution must be judged is not its fame or its splendour or its power, but the quality of the lives lived by the individual members of it. 'I am come that they may have life, and have it abundantly.' I seem to recall a phrase of Aristotle's in which he speaks of a man who lives the life of a vegetable. By this test, the life of a vegetable scores a very low mark. The task of leadership is the enhancement of the quality of life; and in St. Benedict's book there rings more than once the phrase like the tolling of the bell: *In tremendo iudicio facienda erit discussio.* At the great judgment seat of God the abbot will answer for it.

There you have the paradoxical contrast: the humanist of the renaissance with his contempt for human nature, and his doctrine that its weaknesses should be exploited by the prince to the full extent of his needs; the saint of the dark ages with his belief that the task of human leadership is to work with the magnificent best in human nature; to enhance its qualities still further; that in so far as the abbot fails in this supreme task, he is committing the unforgivable sin. 'The Lord is King,' said the psalmist. 'Righteousness and judgment are the habitation of his seat.' That is of course the whole crux of the matter. Is the Lord King? Is there a right and a wrong, independent of the prince or the abbot; independent of the abbot's, or of the minister's regulations; independent of the magistrate or the headmaster, a final standard by which their actions are being judged all the time? St. Benedict thought there was.

Now it is undoubtedly a feature of the present time that the man in authority is more likely to rely on the principles of Machiavelli or Hitler, or at least share the view of human nature which they held, than to rely on the principles of St. Benedict. If we could step back into the seventeenth century, we should find that

the two sides in the Civil War, great though their differences may have been, were yet alike in that the leaders on both sides were Christian leaders in this sense. For Charles I, as for Cromwell, there was an authority over and above their own. Something that might say no as well as yes. For both men, their associates and followers were individuals with immortal souls, individuals whose individual good (in the highest sense) was a matter of supreme concern, something for which Charles, or Cromwell, would be answerable (in St. Benedict's phrase) at the judgment of God. So their leadership was human and personal. The danger at present is not so much in a contrast between Russia and the West in such matters. The danger is that leadership may everywhere become mechanical, rubber-stamp leadership; the exploitation rather than the cure of souls.

The conventional solution to this problem would be found in a series of legal checks and balances. Such a system can now have no effect but that of producing a sort of paralysis. What I wish to suggest is that custom and tradition are in such matters far more powerful, and incomparably more effective, than law. We in the public schools are often laughed at (not by any means without reason) for our customs and traditions. But they do at least illustrate this important sociological fact. A school rule is (I am bound to admit) from time to time broken. A custom is not broken. Custom binds the headmaster as it binds the smallest junior. One of the most important jobs of education is to restore this customary restraint, which is the only effective restraint. It cannot be pretended that we in the public schools have 'got this right.' The point is only that the reign of custom and tradition in them indicates a technique which can be successful when administration by law is not. You may have noticed that in my contrast between St. Benedict and Machiavelli, it was implied that the restraint was religious. I believe that as a matter of history this has been generally, though perhaps not always so. The restoration of sanity in sixth century Greece, with the watchwords of moderation, self-knowledge, the avoidance of pride, reverence for the stranger, grew out, perhaps, of a very deep customary morality rather than a religious morality, though this suggestion would doubtless be hotly disputed. Whether such a thing could be done now, without any definite religious basis, I will not attempt to discuss. Personally I doubt it. But whatever be the truth about it, here is a problem that is vital to our education; the restoration of a sense of restraint and moderation; the preaching of the dangers of arrogance; the revival of the sense of reverence—in relation to man as well as to God. But it would be moonshine to suggest that this is a job that the schools by themselves could tackle. It could be done only if that most powerful of all educational influences, the home, and also the next most important, the vast system of advertisement and commercial amusement were at least not working against them.

The solution of the other problem, that of the control of the powers let loose by technical advances, appears to be bound up with two educational necessities, the need for educating the non-scientist in the ways of the scientists, and the need for creating in the scientists some apprehension of the social implication of his technique. We have before us the prospect of a paper by the President of Harvard University, a distinguished scientist who has recently done most notable work in developing a method for meeting the first of these needs, teaching the non-scientist about science. I will only say here that the subject on which he is to talk to us seems to me to be of absolutely fundamental importance. Nor will I say much about the other question, except just this. I think I should be right in maintaining that the educated man of the eighteenth century still regarded the discoveries of the scientist as being part of his normal reading. It was in the nineteenth century that there started a boiling down and predigesting process. This was the moment of the great cleavage. It came, I think after the publication of the *Origin of Species*, which, like Lyell's *Principles of Geology* was widely read by men who were not primarily scientists. The book sold by the thousand, as anyone who collects the early editions will soon discover. What the development of popular science (and indeed popular archæology in precisely

the same way) seems to me to have done, is to relieve the specialist of obligation to the non-specialist. So we drift apart. The professional archæologist works for a restricted circle of associates to an even greater degree perhaps than the professional scientist. The more esoteric his work is, the greater its prestige value. But what is wanted is that the expert archæologist, not simply the interested amateur like myself, should write books for the general public. And the exclusion of the world outside from the immediate impact of new ideas in science (if not in archæology) is a serious matter. It is true, unfortunately, that most of us will never have any clear understanding of what a new formula worked out by Dr. Einstein means. But until we reach the stage when anyone who has a reasonable intellectual equipment, has some conception of the kind of things that are under discussion, this cleavage, and with it the dissociation between technical development and social control, will last ; and to remove it appears to me to be one of our most important tasks. Any culture that is completely departmentalised tends to a certain crumbling desiccation. The *raison d'être* of this Association, as I see it, is precisely to attack this problem, and to my mind that is one of the biggest educational needs of the age.

QUALITY IN AGRICULTURE

Address by

PROF. H. D. KAY, C.B.E., F.R.S.

PRESIDENT OF SECTION M

IT has become almost a tradition in Section M that the President deals in his address with a general rather than a particular topic. Though as an experimental scientist I ought to look at any traditional method with a questioning though not unkindly eye, I nevertheless propose in the present address to follow this precedent. I shall put before you a few general remarks, which I hope will not be too platitudinous, on what is, I believe, in the opinion of most members of this Section, one of the most pressing and important questions of the day—improvement in farming quality and output in the United Kingdom. Particular aspects of this main theme will be brought forward by others, better qualified than I to make cogent recommendations, later in our sectional programme. I shall illustrate my own remarks mainly by reference to dairy farming.

There is general agreement that in the new circumstances that have followed our tremendous national effort and the expenditure of most of our overseas resources during the war, the maintenance of a tolerable standard of living in this country after 1952 will depend, to an extent which is both appalling and stimulating, on the quantity and quality of food that we can obtain from acres under our own control. The number of these acres is lamentably small in relation to the size of our population—almost exactly half an acre of cultivated land for each man, woman and child in England and Wales, which rises to a little more than half an acre on a United Kingdom basis. The National Farm Survey, 1941–3, showed that for England and Wales the amount of derelict land (that is, good or fairly good farming land that had been under cultivation but which for various reasons, mainly of a physical

or an economic character, had been so neglected or badly managed as to be producing little or nothing of value, though capable of restoration to productiveness) did not reach 1 per cent. of the total cultivated acreage. To-day, it must be still smaller in amount. The total area of marginal land—a term not easy to define but which for present purposes may be taken as including not only derelict but other land, much of it poor, which has never been effectively cultivated, but which by drainage, protection from the sea, clearing of bracken, heather, scrub, copse or boulders, levelling, etc., could be brought into a sufficiently useful state to permit of its being farmed in the future without financial loss—is considerably greater. Its area may be 5 per cent. or even more of the area of the land actually cultivated in England and Wales, and probably at least twice that percentage in Scotland. Its substantial improvement mainly for meat producing purposes, so ably advocated recently by Moses Griffiths, undoubtedly entails the employment of no inconsiderable volume of scarce labour, machinery and capital. Much of the land so improved would need expenditure on maintenance at a higher rate than that required for an equal area of land at present under cultivation, but the psychological effects, both on the nation at large and on the farming community, of a campaign for increasing our cultivable acreage would, I believe, be astonishingly heartening.

Though the agricultural potentialities of every acre of our country, whether at present cultivated or marginal, must have been carefully scrutinised time and time again in the course of the centuries by generations of enterprising farmers and landowners, yet fairly recent developments in reclamation science, technique and machinery, including the production of new strains of herbage and other plants suitable for hilly or salty land, combined with our sombre national economic position, make an early and thorough re-examination of the marginal land question —preferably, as Sir George Stapledon has suggested, by a number of decentralised regional authorities—a matter of high priority.

Nevertheless, were every pole of derelict land brought back into cultivation and the utmost—consistent with economic sanity—done with other marginal land, we could not raise production at home from its wartime peak of some 40 to 42 per cent. of our food requirements to the 50 per cent. or even more that is our first objective, without growing at least 20 per cent. more food per acre on a very substantial proportion of our *present* cultivated area.

But, it may be asked, are not quality and efficiency [1] in farming so high in this country that on existing cultivated land there is no more than a small margin left for improvement? Are we not already close to the upper limit of the productive capacity of such land? Shall we not ruin fertility if we press our overworked acres, wearied by too insistent wartime and post-war demands, any harder? As was pointed out by Walter Elliott in an outstanding paper to this Section in 1947, the yield per acre of various crops in Britain, and the monetary value of the output per man per year are very high compared with those in the majority of overseas countries. They were far higher in terms of output per acre than in the New World countries, and only Denmark and Holland (as regards yields per acre) and New Zealand and Australia (as regards cash output per agricultural worker) were ahead of us. A. W. Ashby has stated that, as regards number of persons fed per person engaged

[1] I ought, perhaps, to make it clear that in this address the words quality and efficiency will be used for the most part with their usually accepted connotation, and not with the rather specialised meaning, especially for 'efficiency,' that is sometimes applied by the economist. An 'efficient' farmer, in the sense in which I shall use the term, is one who is turning out a thoroughly satisfactory quantity of good quality products of the type needed for the national economy, whilst at the same time preserving the full capacity of his land to continue such production indefinitely, and is making a reasonable living out of the process both for himself and for those he employs. The farmer 'of quality' is efficient in this sense, but he is something considerably more. He is a specialist agriculturist with all the technological knowledge and practical skill required by his avocation, with judgment, forethought, style and the keenest interest in his work. He is steadily increasing his yields, he is raising the quality of his products, he is improving the fertility of his soil, he is, where opportunity offers, bringing waste or marginal land into cultivation. The monetary incentive though of importance to the quality farmer as to any business or professional man, is not his master and is usually not his main incentive.

on the land, Britain is only surpassed by Australia and New Zealand. It has been recently stated that British agriculture is now more highly mechanised than that of any other country in the world. In general, it is not unfair to say that the quality of our farming is high compared with that in most other countries. Nevertheless, every British agriculturist knows, and every non-agriculturist may derive some small comfort from the fact, that the reply to the questions just posed is that we could undoubtedly produce far more food at home, without any loss but rather with a gain to our soil fertility, were existing scientific knowledge and modern technique made full use of by a substantially greater proportion of our farmers than at present. Whether he has or has not read the report on the National Farm Survey, every observant person in British agriculture is well aware that on far too many farms quality and efficiency leave much to be desired.

The legislative approach

What, in view of the importance and urgency of raising the average quality of farming in this country closer to that of the top 10 per cent. of farms, can we do about it? Let us consider first the legislative approach with its inducements and penalties. The Agriculture Act, 1947, ' an Act to make further provision for agriculture ' must be for all those concerned directly or indirectly in our basic industry one of the most important pieces of legislation on the Statute Book. No humble research worker like myself would hazard a guess as to the proportion of real farmers in the country who are familiar with all its provisions, who have plumbed, shall we say, the full depth of the alleged feathers in this bed. It is reasonably safe to assume that there may be one or two here to-day who have not. May I quote from it two statements that have relevance to the present discussion? In Part I of the Act, it defines one of its purposes as being to promote and maintain ' a stable and efficient agricultural industry capable of producing such part of the nation's food and other agricultural produce as in the national interest it is desirable to produce in the United Kingdom and of producing it at minimum prices consistently with proper remuneration and living conditions for farmers and workers in agriculture and an adequate return on capital invested in the industry.' For present purposes the operative words in this quotation are ' efficient ' and ' national interest.' In Part II of the Act, the part dealing with ' good estate management and good husbandry,' it states that ' the occupier of an agricultural unit shall be deemed to fulfil his responsibilities . . . in so far as . . . the occupier is maintaining a reasonable standard of efficient production, as respects both the kind of produce and the quality and quantity thereof, while keeping the unit in a condition to enable such a standard to be maintained in the future.' Here again efficient production and quality of produce are prominent. It will be remembered that if the occupier of an agricultural unit is not fulfilling his responsibilities to farm the unit ' in accordance with the rules of good husbandry,' the Minister has ultimately the power of dispossession.

It is not proposed to discuss here the sharpness or otherwise of the teeth of this legislation, though time seems to be showing that they are a good deal blunter than was intended. What is to be emphasised is that for the first time, I think, in our history, efficiency in technique and quality in products were specified in legislation as the main objectives of home agriculture, the non-attainment of which is subject to penalty. In their attainment, ' practice with science ' are indeed the two major factors. The ' practice ' factor I shall not dwell on in this address. It is essentially managerial ability, the faculty of planning and effecting the business of the farm, including relations with labour and the outside world, so that materials, labour and time are fully and economically used and the whole farm works together as a friendly unit. It depends essentially on inborn nous, or gumption, a quality difficult to define but which we usually recognise when we see, which some lack entirely but which, when present in embryo, is capable of training and development. The other factor about which I shall say more, the ' science ' factor, is, in short, the adequate application of modern farming technology. These two interconnected factors, and not the inherent productivity of the land or

the fear of exhausting the fertility of the soil, are the limiting factors in achieving the large increase, which the Minister demands and the nation so urgently needs, in the output of food of good quality from most farms in this country.

Efficiency in technique, quality of products and the rules of good husbandry are concepts not easy to translate into quantitative and measurable terms. Since, however, they are statutory requirements they need further definition, if possible definition containing little if any of the merely subjective. Here I may have to rush in where, if I may combine two allusions, the economist angel fears to tread other than delicately, knowing well that he may be hewed in pieces. However, at risk of the same fate, I hope to advance one or two innocent remarks on efficiency and quality and illustrate and support them mainly from the field of what is at present our major agricultural activity—farming for milk production.

Quality of farm products

In assessing agricultural efficiency, it is clear that both quantity and quality of products must be taken into account. These two aspects are somewhat akin to the capacity and the intensity factors common to so many physical measurements, such as the evaluation of heat, electricity or acidity. In agricultural products, the intensity factor must be measured by the extent to which the final product coming off the farm meets the qualitative requirements of the consumer, possibly in terms of nutritive value (e.g. milk or dried grass), or in terms of the amount of some industrially desirable substance or material per unit of product leaving the farm (e.g. percentage of sugar in a sugar-beet crop), or as assessed by skilled graders (e.g. wool or cheese).

In the recent past, in regard to many farm products, the extent to which the product fits the purpose for which it is intended has not infrequently been regarded as of less importance than has yield, partly because of the seller's market and partly because quality has always been more difficult to measure than quantity. Lord Kelvin said many years ago that if you can measure that of which you speak, and can express it by a number, you are beginning to get to real grips with your subject. Quality of silage, of wool, of milk, of a dairy bull, of a pasture, of a baking wheat, and of other important agricultural products, intermediate or final, require methods of measurement that, if available at all, are usually not available on a farm. An attempt at assessment may be made, and often is made, by rule of thumb, or by long experience, but not always a successful attempt. In the case of the dairy bull, for example, quality, which takes at least four or five years to measure, has as a consequence been all too frequently in the eye of the beholder. Further, whilst yields per acre or per man can be measured with almost equal accuracy by individuals X, Y or Z, the market quality of the product may depend much more on fluctuating subjective opinion. More scientific research is undoubtedly needed to provide a clearer definition of quality in many agricultural products, and replace rule of thumb by some kind of numerical measurement, preferably one that the farmer can himself use.

Although quality of most farm products is at present less easy to define and to measure than quantity, it is by no means a rather highbrow requirement about which agriculture need not seriously concern itself. Its pursuit is indeed quite fundamental to progress in agriculture and to better living for the consumer. Though a stable civilisation can hardly begin until the bare quantitative needs for food, clothing and housing have been met, and though the compulsions and stringencies of the war and post-war period have played sad havoc with quality in so many directions, yet as regards agricultural products, one still has hopes that within our lifetimes British beef, pork, bacon, poultry, cheese, fruit and vegetables may get back to pre-war standards both in the capacity and the intensity factors ! We have at least the underlying scientific knowledge to produce even better quality than pre-war, and organised agriculture would be most unwise, in view of the trend of events, not to put quality in the forefront whenever production policy is being considered. It is true that a high-quality product usually costs more than the average or poor product, but science is continuously

making it easier to produce a high-quality product at a moderate cost.

What constitutes farming efficiency?

The phrase 'farming efficiency' as used in a somewhat specialist sense by my economist friends, does not mean the same yesterday, to-day and for ever, or from one economist to another, or even the same to the same economist at different times. It cannot be considered without reference to changing environmental circumstances and the method of assessing it must vary with those circumstances.

In British agriculture to-day the circumstances are that planned economy armed for indirect control with price-fixing powers, but having relinquished the direct control which was exercised during the war, is, with the concurrence of all the political parties, endeavouring to provide a stable basis for increasing production of certain stated commodities. The agricultural expansion programme of 1947, with subsequent minor modifications, asked of the farming community that, along with increased production of milk, it should turn out *the maximum quantity of which the industry was capable of certain dollar-saving products*, particularly (in order of increased production over 1946–7) the livestock products pig meat, eggs, beef and veal, mutton and lamb, and the crops linseed, wheat, barley and oats. The need for continued maintenance of such increased production was to be borne in mind. The farmer was asked to produce these commodities at prices so adjusted as to provide proper remuneration and living conditions for himself and his employees, and an adequate return on capital invested. Though the land remains and flourishes, buildings and equipment, like those who own and use them, grow old and obsolete, and it was intended that a steady renewal or replacement of old buildings and of live and dead stock, together with improvements in services and other facilities, should also be financed over a number of years out of the guaranteed returns. But in spite of the not unfavourable prices offered, the average farmer could hardly hope to reach all these desirable objectives without considerably increased efficiency in his farming.

As regards milk, the dairy farmer was expected to produce sufficient of this commodity to make a rationing system for liquid milk consumption unnecessary at any season of the year. This part of the programme has almost been achieved, though another 20 per cent. increase in total milk production will be required before the nutritionists' consumption standards are reached. Further, as the Chairman of the Milk Marketing Board has recently stated, there should be sufficient milk produced, over and above that required for liquid consumption, to permit a fair allocation to all milk manufacturers, particularly those manufacturing milk products in the higher price-category markets.

Whether or not the specific products mentioned in the agricultural expansion programme were the right ones on which to concentrate is a question of extraordinary difficulty. Much thought must have been given to it by the Government authorities, with full figures covering supplies and requirements before them. The programme, with minor changes since 1947, is obviously aimed at reducing, in relation to food supplies, our economic rather than our military vulnerability. As regards the latter type of risk, the best insurance, though a costly one, would surely be to store a year or two's supplies of certain storable key products such as wheat (and possibly other grains), sugar, fats, dried milk, animal feeding stuffs, potassium salts and phosphates for fertilisers. Special buildings would be required for the storage of some of these commodities, and most would require regular turning over. It would take us too far from our present topic to discuss this question in detail, though it obviously has most interesting scientific aspects.

For present purposes, I simply wish to point out that official guidance as to what, for national purposes, constitutes farming efficiency, at least till 1952, was given in 1947 by the Act and the expansion programme, namely maximum output per cultivable acre, within the limits of available man power and other resources, of certain specified products. Quality of these products, rather like a pale spirit called up but ill-defined, floated uncertainly in the background.

The immediate expansion programme is

not, in the ordinary sense of the words, a challenge to agricultural science. Rather is it a challenge to the *science of applying existing knowledge as rapidly as possible* to increasing the productivity of the large majority of the relatively small units of an inherently conservative industry. On a long range basis, nevertheless, the demand is, it seems to me, clearly put to science to solve the fundamental problem of producing human foods of high nutritive and gustatory value, with considerably less expenditure of feeding stuffs, acreage and home resources generally than even the best methods of conversion via the animal at present involve.

Measuring output efficiency

The assessment of true farming efficiency, if in fact there are not many different partial assessments which can hardly be integrated and which largely depend on the relative weight given by the assessor to the variables involved at the time the assessment is made, has in the past been frequently attempted in monetary terms. Value of saleable output per acre, or per man, or profit per cow, or total income per whole-time person engaged, or even more complicated bases of assessment such as the net import-replacement ability in £ per acre, have been, and still are being, used. The yardstick has been the £. Unfortunately, with the yardstick, as in the past few years, subjected to such arbitrary changes in length, with acreage payments and other fluctuating subsidies, with controlled prices for certain farm products, with exchange rates subject to sudden alteration, one cannot help feeling that the assessment of farming quality in terms of monetary values has lost any precision it may have had and has acquired something of the flavour of a mere intellectual exercise. No one, fortunately, even endeavours to evaluate medical efficiency in terms of monetary profits per patient per year.

In dealing with an activity of such outstanding national importance as agriculture, particularly at the present critical time, it may well be that the shortcomings of the purely monetary or profit-making approach loom unduly large, and that there is, in fact, no other practical or practicable basis of assessment. But in view of the objectives of the Act and the expansion programme, may we examine for a moment a possibly more fundamental value, namely the physical quantity of production per acre—a value which now affects us all, whether farmer or mere consumer, so seriously?

If, in any district, production standards that a reasonably proficient farmer should obtain in saleable output per acre for most of his farm products, including livestock products, can be set up, it should be possible, taking his soil quality, local climate and any other special advantages or handicaps into account, to compare the achievement of any farmer in that district against the standard, and give a shrewd and at least semi-quantitative assessment of his yearly production efficiency in terms of a normal local or even national standard. In such an assessment on a livestock farm, the quantity of feeding stuffs brought on to the farm from without must in some way be accounted for. Purchased fertilisers, on which grassland production for livestock as well as yield of arable crops depend, need not be brought into this calculation, unless the farmer is for some reason unable to obtain the quantities which he regards as adequate.

Some nine years ago, a production-recording scheme, the main purpose of which was to give the individual farmer a semi-quantitative idea of how high up he stood on the technical ladder, how he was progressing from year to year, and also to afford a basis for advice was devised, as a result of a suggestion from L. R. Bomford, and put into practice in parts of Hampshire. Standard yields per acre of arable crops, vegetables and livestock products which should be obtained in the district by an occupier reasonably skilled in husbandry, farming during an average year, were worked out. Livestock products presented most difficulties and it was found convenient to estimate certain of them in terms of cow units; thus, two summer fattening cattle or five arable or semi-arable ewes were regarded as equivalent to one cow in their demands on the holding. The awkward question of how to deal with purchased feeding-stuffs was met by adding to the acreage of the farm the area which would normally be needed to grow them.

This scheme was, and still is, on a voluntary basis ; the work is done free of charge to members and the results, obtained at present from about 100 farms, are confidential. If a farm reaches approximately standard yields it is given a marking of 100 per cent. As regards type of soil, it is found in practice that no correction is needed for any but farms on the chalk, which are found to average 15 per cent. lower in efficiency rating than farms on other types of soil.

For present purposes, some of the most important general findings are : that in spite of the fact that the members of the scheme must be a selected population above average in quality, their efficiency as averaged out over the past four years is only 88 per cent., that there is a very wide range of efficiency from as low as 20 per cent. to as high as 145 per cent., that there is a 'hard core' of farmers in the 20 to 80 per cent. range who seem to be quite unable to improve their methods, that many of the higher ratings from 100 per cent. upward are newcomers into the industry, that productivity per acre rises with intensity of stocking and that as a general rule productivity and profitability go hand in hand. 'The dominating factor in output is the quality of the management and not the land.'

The standard normal yield for milk production is a yearly output of 200 gallons per acre, of meat 2 cwt. per acre, of cereals 18 cwt. per acre, of potatoes 6 tons per acre and so on. These yields are, perhaps, a little on the low side for the really good farmer ; category A might well be given on 110 or 115 per cent. and above. Another possible criticism is that *quality* of product does not seem to have been taken into account, though in actual practice some correction may be made for quality. It seems to me that in the Hampshire scheme—though it is no doubt capable of improvement—we have the nucleus of a method for the assessment of farming efficiency on the nationally significant basis of physical output per acre. If it were possible to combine this method with, or supplement it by, the method used recently by C. H. Blagburn (i.e. net import-replacement capacity per acre, measured in £), we should, I believe, be in a position to make an effective assessment, very close indeed to what, under present national circumstances, is required, of farming efficiency.

Whether associated with or apart from monetary basis of measurement, any sanely devised productivity - recording scheme, accompanied or not by a modest degree of publicity (even if names of any but the leading producers are withheld) is likely, human nature being what it is, to have a stimulating effect. Most human activities are fostered by a little honest emulation, and as the success of the milk-recording scheme (which I shall refer to again in a moment) has shown, the effects may be very far from negligible.

Quality in dairy farming

Since milk production is, when measured in terms of numbers engaged or in cash value of product, by far the most important of agricultural enterprises in this country, it is not unreasonable to give it particularly careful scrutiny.

Outstanding is the astonishing variation in efficiency, by whatever yardstick it is measured, from farm to farm, even of similar type, in the same district with, apparently, closely similar advantages and handicaps. To revert for a moment to the Hampshire scheme, outputs per acre vary from below 100 gallons to as much as 470. Again, in a recent report by R. S. Cook on milk production in 67 farms in the Southern (Advisory) Province, the average yield per cow per annum varied from 990 gallons in one herd to as little as 282 in another. From the cost-of-production standpoint, the variation was from over £80 per annum per cow down to under £40 ; food costs per gallon varied from over thirteen pence to under sevenpence ; profits per cow per year ranged from as much as £55 to under £5. Relevant also is the fact that, whilst in herds recorded under National Milk Records (covering England and Wales) the average yield per cow per lactation of 305 days was, in 1948–9, nearly 740 gallons, the average yield for the whole of the milk-producing herds in England and Wales, estimated as nearly as possible on the same basis, was under 560 gallons. Although only 20 per cent. of the total number of cows whose milk is sold are recorded under National Milk Records, it

is clear that there must be a very large proportion of cows and also of herds, with individual lactation yields well below 500 gallons per cow. Some of these herds may be those of mixed farmers whose interest in milk production is secondary, but making all allowances for breed differences, for genetic shortcomings in individual animals (largely controllable by culling as an immediate remedy and an adequate breeding policy as the long-term cure) and for other factors there is no doubt that variations in the quality of dairy herd management are very wide and that we cannot, as yet, be proud of the average standard achieved.

The typical predominantly dairying holding in England and Wales has 65 acres of crops and grass and 4 acres of rough grazings. What might we reasonably expect in output and quality of product from an intelligent dairy farmer working such a holding on modern lines?

To start with a negative, I think we cannot expect him, against the background of the increased social amenities and periods of free time which his neighbours in country or town alike enjoy, to continue indefinitely the semi-slavery of the 7-day week and 365-day year which was, and still is in many cases, the lot of the smallish dairy farmer. Here, modern scientific developments in technique should help him greatly, since he should on some 65 to 70 acres of average land, i.e., land which is reasonably accessible, adequately drained and neither too light nor too heavy, be able to maintain, not the present average of 12 to 14 cows, but a more remunerative unit of nearly twice that number. For such a unit, he would require, and should be able to pay adequately, at least one other competent worker besides himself. With this number of cows, machine-milked in properly designed premises, it should be possible to arrange for quite satisfactory working hours, days off and adequate holidays for both farmer and cowman. As regards feeding stuffs, modern technological developments in improved leys, adequate use of fertilisers at the right seasons, rotational grazing with electric fences, the extension of the green-feeding season into the winter months by using modern kales (improved in their protein content by using ample nitrogenous fertiliser), the making of quality silage from grass-clover leys or from cereal-legume mixtures grown *ad hoc*, should enable the farm to be almost self-supporting as regards nutrients. A few tons of concentrates would be highly desirable, if not absolutely necessary, for the best-yielding animals at the height of their lactation. The foregoing feeding stuffs, together with 40 tons or so of well-made, home-grown hay should give ample nutrients as well as enough roughage to prevent a fall in compositional quality of milk either in late winter or at the beginning of spring grazing. H. G. Sanders has recently shown how small is the overall shortage of protein in relation to present livestock requirements and how a quite small increase in protein from grassland—something of the order of 4 per cent.—would give us all that is required. On the efficiently run dairy farm, the quality of the home-produced herbage, young-grass silage, and other crops for milk production would be such as to give not only ample protein, but protein of the right kind for the lactating, herbivorous ruminant. The winter feeding period should extend over not more than three and a half months or perhaps four in the bleaker districts.

One of the largest items of expenditure would be for fertilisers, not only nitrogenous manures but those containing lime, phosphate and potash, to minimise the drain of high milk-production on soil mineral reserves and also, of course, to improve yield and quality of farm-produced feeding stuffs and of any small acreage of cash crops that might be grown. Efficient management with adequate fertilisers should enable ample nutrients to be produced on 65 acres, to allow, with a modest purchase of concentrates, of all replacements of a herd of 25 cows being raised on the farm. If the herd was within range of an artificial insemination centre—and such centres, with bulls of well above average quality, now cover practically all dairying districts of the country—there would be no need to keep a bull, thereby saving food, accommodation, trouble and expense, and allowing the replacement of what is now, for the smaller herd, a liability, by a real asset in the shape of another cow.

The breed used would normally be either one of the single-purpose dairy breeds or a first-class dual-purpose strain. As

regards producing milk nutrients from feed nutrients, the knowledgeable dairy farmer would be aware that breeds vary somewhat in efficiency. If he started with a non-descript herd, it might take several bovine generations to get the type of animal he required. The herd would naturally be tuberculin-tested, vaccinated with strain 19 against contagious abortion, and under close veterinary supervision in respect of mastitis and other bovine diseases. It would be dehorned. With reasonable precautions the average length of life of cow in the herd would be not less than seven lactations, though in the earlier stages of herd-building active culling would doubtless reduce this number. The average period between calvings would be not more than 390 days. The herd would, of course, be milk-recorded—an absolute essential for any quality dairy herd—and that not only for milk yield. Of the cows at present in milk-recording schemes, only about one-quarter are fat-recorded, but in an efficiently managed herd not only fat, but also non-fatty solids would be determined. Facilities for this latter purpose, where they do not at present exist, ought certainly to be provided.

An average yield would be 750 to 800 gallons per lactation of 305 days, obviously depending somewhat on the breed of cow. The 24-hour bulked herd milk should normally contain not less than 3·7 per cent. fat and 8·75 per cent. of non-fatty solids. If these figures could not be attained otherwise, then a few Channel Island animals might have to be kept.

If we assume for present purposes that the fairly small area devoted to cash crops would be about equal to the area on which the purchased concentrates would be grown, the yields per cow and the acreage just mentioned would give an output per acre per year of some 260 to 280 gallons, say, between 320 and 350 lb. of milk solids, a figure well within the productive potentialities of most dairying areas in this country, if quality of management of grassland and of herd is up to good modern standards. Much higher yields per acre are, of course, being obtained. Some land I know particularly well produced over 450 gallons per acre in 1949, during a part of which a fairly severe drought was experienced. The stocking on this land from April to October, during which no food not grown on the land was used, was at the rate of one cow in milk per acre. With a normal season there is no doubt that far greater output per acre could be obtained even on this rather light land; a few commercial farms elsewhere have, in fact, exceeded this yield. It will be of great interest and value when a body of statistics now in course of collection showing the yield, on specialist dairy farms, of milk per acre per year from different types, quality and ages of leys, with records of management methods, becomes available.

Buildings, water supply, provision of light, heat and power, all come into the picture. One cannot imagine an efficient industrial unit, however small, that was without reasonably adequate buildings, or where labour was wasted by unsatisfactory layout, or where essential services were missing or grossly inadequate, nor can a dairy farming unit reach quality standards under present-day conditions till these primary requirements are met. As regards power, a tractor with mounted implements would be a practical necessity on an up-to-date, efficient dairy farm of 65 to 70 acres, in addition to a fixed power unit or electricity supply for machine-milking, cooling, production of steam for sterilisation of utensils, food preparation, etc., on the farm. The higher efficiency, however measured, of many dairy farmers who have begun operations of recent years, as compared with that of a considerable proportion of the older-established farmers, is due in no small part to their having, in general, better designed and more modern plant and services, often provided by fresh capital and not infrequently supplemented by modern technical knowledge and ideas.

Lastly, our quality dairy farmer will attend a short refresher conference, extending over at least a fortnight, every four or five years, to bring himself up to date on recent applications of science. His skilled stockman will do the same, so that, each two or three years, new knowledge, to be considered critically and applied with understanding, will get to the farm and not merely remain on paper. In short, to quote Sir Thomas Browne of Norwich, ' every man must study his own œconomy and adapt such rules unto the figure of himself.'

Quality in milk

Like efficiency in farming, quality in farm products may be assessed on several different bases, and in no farm product is this exemplified better than in milk. Keeping-quality, compositional quality (which is closely allied to nutritional quality), quality as regards freedom from possible pathogenic organisms, quality for buttermaking, for cheesemaking or for dried-milk manufacture, quality for pasteurisation, are requirements called for to a greater or lesser degree by different types of milk buyers or consumers. As regards keeping properties, if, as is true nowadays for the enormous majority of producers, our quality dairy farmer is producing milk for liquid consumption, good keeping-quality, particularly keeping-quality in hot weather, will be one of his main objectives. The National Milk Testing and Advisory Scheme of the war years showed the very wide variation between milk producers in this important aspect of quality. Using dye reduction tests as an approximate index of true keeping-quality, milk was classified into three categories A, B and C, which, in effect, amounted to good, mediocre and poor. Over 50 per cent. of farmers, whatever the season of the year, produced milk of uniformly good keeping-quality, despite unfavourable weather conditions and various wartime handicaps. Others, not remotely dissimilar in facilities, seemed unable to produce in the warmer weather other than B or even C category milk. We could, I think, expect supplies of milk from our efficient dairy farmer (using modern technique of regular steam-sterilisation of all utensils and apparatus that come into contact with milk, and of adequate cooling and maintenance at a temperature below 50° F. of all milk till it leaves his farm) to have a subsequent keeping-quality, even without pasteurisation, of at least 24 hours at summer shade temperatures.

As regards freedom of milk from pathogenic organisms, I have indicated that the competent dairy farmer will in his own interests keep his herd free from tuberculosis, will vaccinate against contagious abortion and will take all possible measures against udder infections. No cowshed work (nor handling of milk subsequently) will be done on his farm by anyone suffering from, or suspected of suffering from, an infectious disease.

Another aspect of quality—compositional quality—is nowadays, with uniformity of payment per gallon for all except Channel Island milk no matter what the composition of the milk may be, frequently overlooked. Unfortunately, the average compositional quality, which may for practical purposes be regarded as a measure of the nutritional quality, has tended to fall off in recent years—a slow, somewhat fluctuating but quite indubitable decline in both fat and non-fatty solids percentage over perhaps two decades. Not as a result of breeding for quantity, but largely as a result of breeding for quantity *with little or no regard for quality*, there are to-day amongst our better-yielding herds too many cows with a milk composition that will hardly bear thinking of and which no farmer of quality would wish to keep in his herd. There has also been, of late years, a rather sharp fall in milk composition in several areas of England in the winter months, a fall mainly in non-fatty solid content, which is completely reversed when the cows go out to grass in the spring. May I, at the risk of belabouring the obvious, recall that the main reason for the existence of the dairy farmer and the milk industry is that cow's milk is an excellent human foodstuff; if the compositional quality and hence the nutritional value of the milk is allowed to decrease, it is a matter of the most serious import for the producer and consumer alike.

At present there is no direct financial inducement to the producer to maintain any standard of nutritional quality in the milk he sells, and he cannot be legally convicted for selling milk of poor quality, no matter how poor it may be, if it is as it came from the cow. Nevertheless, our quality dairy farmer will certainly take all possible steps in applying modern knowledge to maintain at an irreproachable level the compositional quality of his milk, despite the fact that for the present he is in general being paid for volume only. Milk *quality* recording, and breeding preferentially from strains giving quality as well as yield, feeding the cow a supplement of concentrates for a few weeks before parturition—which usually improves quality as well as yield—giving the animal

in milk entirely adequate quantities of energy (' starch equivalent ') assessed on a milk-yield-plus-maintenance basis, making use, in relation to winter feeding, of modern methods of preparation and storage of home-produced grassland products and other means that need not be detailed here will all assist the efficient and conscientious farmer, anxious to keep up the consumer-value of his milk. He will thereby be able to maintain his herd in such 'fettle' that when, as it assuredly must in the next few years, quality payment comes along, he will not find himself at a serious and difficultly remediable disadvantage. What is even more important, he will be giving the consumer, who has no means of judging the nutritional quality of the milk he buys, a square deal. The unwitting householder, particularly in the less well-off family, is frequently leaning heavily on his milk supply for keeping the nutrition of his children at a reasonable level, and it is unfortunate that during the winter months, when the human diet, and particularly the child's diet, is more dependent than during the summer and autumn on the supplementary value of the proteins, calcium, vitamin A and other vitamins of milk, the nutritional value of this product from so many herds should reach its minimum.

The remarkable effect of a price-incentive, combined with a reasoned case put clearly to the farmer, in improving the quality of an agricultural product has been demonstrated of recent years in connection with the drive to eliminate tuberculosis from dairy herds. Since 1943, when an extra 4*d.* per gallon began to be paid for 'T.T. milk,' the proportion of milk coming from tuberculosis-free herds has increased from about 8 per cent. to 29 per cent. of the total national supply. Although the quality farmer will as usual be ahead of the times, it is unlikely that the majority of his fellow producers can be expected, without either an appropriate price-incentive or the risk of an economic penalty, to produce milk of high compositional quality throughout the year, since on the whole it costs more in feed per gallon to do so. Nevertheless, it seems reasonable to suppose that the price-fixing mechanism now in existence could with minor modifications be used to provide such an incentive. The result would be not only better quality milk for the consumer but better cows and generally increased efficiency of production.

Quality dairy farming and meat production

I have dealt in some detail with the qualities desirable in milk. Other farm products might be examined for quality in a similar way. As regards meat, somewhat different considerations apply, though improvements in quality and quantity of home-produced meat, particularly beef, are in part bound up with rising efficiency in dairy farming. Quality as well as quantity of home-grown beef has suffered because of the priority given, and rightly given, to milk. Nevertheless, most students of the question would, I think, agree that, with the substantially increased yields of milk per cow and per acre that science has now placed within the grasp of the competent dairy farmer, a gallonage of milk—say 2,000 million gallons per annum for the United Kingdom—that would satisfy the nutritionist and at the same time provide a reasonable modicum for manufactured milk products, could be produced from a very appreciably smaller acreage than at present. This could be done, moreover, with no increase in the *total* number of cows, and with little if any increase in concentrated feeding stuffs from abroad. A steady move in this direction, which would be accompanied by a concentration of cows into specialist dairy herds of different sizes should progressively liberate more good quality land both for the home-grown cereal expansion programme and for providing, preferably on the farms which had given up dairying, more area for other types of animal production than milk. Reclamation of marginal land would still further increase this area. The proportion of quality beef should rise steadily and that of cow beef steadily fall, a fall accentuated by the longer life of the dairy cows maintained under healthy conditions by specialist dairy farmers.

The problem of improving, from home resources of feeding stuffs, pork and bacon production and quality may receive a partial solution from some recent developments of a feeding system in which the small cereal-plus-fish-meal dietary nucleus (the fish meal being of sufficiently good

quality to provide all the ' animal protein factor ' required) is supplemented by home-grown fodder-beet of high dry-matter and low fibre content. There is undoubtedly need for research on methods of raising quality pigs entirely on home-produced feeding stuffs. In this connection, I have wondered for some years whether the nutritive value of potatoes for stock feeding could not be much improved if our potato breeders found it possible to incorporate into the modern high-yielding but protein-poor tubers the appreciably higher protein content of some of the lower-yielding strains imported some years ago from South America. Further, we have not yet seriously set about the problem of growing at a reasonable cost fodder-yeast for pigs and poultry from cheap carbohydrate plus inorganic sources of nitrogen and phosphate, either at home or in some tropical colony. A large industry of this kind might be developed in Jamaica or elsewhere, using some of the by-products from sugar manufacture or even using crude sugar, provided that a correspondingly large demand for dried yeast, with its high content of digestible protein and B vitamins, its stability when kept dry and its other valuable commercial properties, could be fostered. One-thirtieth of the money spent on the ground-nut scheme would probably put this industry on its feet with, at home, great benefit to animal and fat production, and abroad, to Jamaican economy. There is no doubt about the high nutritional value of yeast for pig, fowl or dairy cow, provided a suitable amount of ground limestone is added to the diet. It would appear also that valuable feeds derived from fish by-products and from gluts of fish might be more thoroughly exploited and there are similar, though quantitatively more limited, possibilities regarding whale by-products.

Is the good young farmer being kept out?

In any population of living organisms, physiological quality varies. In a large population it varies greatly. Thus, in our 3,400,000 dairy cows and heifers in milk or in calf in England and Wales, for example, very many require culling if milk-producing quality in this and in future generations is to increase. Similarly, in our present population of some 220,000 full-time farmers there is in the broad Gaussian distribution curve of efficiency a substantial tail of which the quality is unmistakably below the minimum standards which our national needs, both now and in the foreseeable future, demand. It is, however, highly unlikely that in any national régime which would be tolerable to our generally long-suffering community, more than a very few of the critically poor-quality ' hard core ' would be dispossessed in peace time. Nevertheless, as an agricultural correspondent of *The Times* recently wrote, ' if we could only eliminate our 10 per cent. of bad farmers, displace them by reasonably good farmers, and raise the standard of the great middle group by no more than 10 per cent., we should have gone a long way towards solving our food production problem.' (It will be remembered that the National Farm Survey of 1941–3 graded 58 per cent. of farmers as A, 37 per cent. as B and 5 per cent. as C. The last two grades were both regarded as unsatisfactory. The 1941–3 figures are now out of date, but the general picture to-day cannot be far different.) He further says ' the machinery for dispossessing bad farmers under the (1947) Act is, in practice, so slow and cumbersome that both landlords and county agricultural executive committees hesitate to set it in motion.'

On the other side of the picture we have a state of affairs that at present does not exist in any other skilled avocation in this country. We have, as a guided guess—exact figures are hardly possible—probably not less than 1,200 and perhaps as many as 2,000 scientifically and practically trained young agriculturists, most of them anxious to farm, who have, under present inflated price conditions, little or no prospect of being able either to purchase or to rent a suitable holding. If by some means these keen, energetic and knowledgeable young men or even, say, the best 1,000 of them could be given a real opportunity to farm for themselves, many of their farms would become in the course of a few years missionary centres for the diffusion of quality farming.

Modern farming, no less than medical or veterinary practice, or engineering, demands to-day a high level of scientific training and practical skill in its entrants, if

its quality in the future is to meet the needs of intensive national food production. Farming must become, in fact, if not in name, a profession for which the same careful and thorough preparation is required as in other nationally important professions. I wonder what our reaction would be if a substantial proportion of our better young medical graduates or our young qualified engineers were not drafted into the avocation for which they had carefully prepared themselves—many at considerable expense to the State—but were forced for a living into selling stethoscopes or hawking spanners?

The chartered farmer

It seems to me that a contribution to the long-term solution of the problem of making substantial improvement in the quality of our farming would be that in future some certificate of competence from a national registration authority should be a requirement before a man (or woman) were allowed to farm on any but a quite small scale. In fact, under the provisions of the Agriculture Act, 1947, there is already some approach to acceptance of this principle as regards future smallholders. Thus: ' . . . a smallholdings authority shall not let a smallholding to any person unless they are satisfied that the said person has had sufficient agricultural experience to render it likely that he is or will become qualified to be a farmer on his own account . . . " Even at present anyone producing milk for sale has to be licensed; though there are very few stipulations before a licence is granted. For future licensees, an acceptable minimum standard of training in dairy farming might well be one of the conditions. The principle might be extended to any holding, whether or not it was used for dairying, of over, say, 100 acres; it could with great advantage be required that any new owner (or his manager) or the new tenant should possess a quasi-professional qualification or its equivalent. Merely to have sufficient capital to buy a farm, or to enter a farm as a tenant, would not be enough. The essential requirement would be either a nationally accepted qualification following a period of intensive training in agricultural science of not less than two years, subsequent to school certificate, together with practical experience on a farm over a like period, or a recent history of successful modern farming deemed adequate by a competent central registration authority which might be established under the ægis of an independent organisation such as the Royal Agricultural Society. Such a chartered entrant might be given borrowing facilities at very low rates from a special fund, to provide him with all the necessary capital for efficient farming. Capital so borrowed would be repayable, with all proper precautions both from the borrower's and the fund's standpoint, over a prescribed number of years. In return, the entrant would be required to keep specified accounts and records of production, to undertake not to delegate his tenancy or dispose of his holding without authority, and (an important stipulation) would agree to retire before his sixty-fifth birthday!

Up to this point, I have dealt mainly with quality in relation to farmers and farm managers, but there is no doubt that without skilled, responsible farm labour, much energy, capital, time and produce will be wasted and quality farming become virtually impossible. Fortunately, in most parts of this country, the farm labour situation at present appears to be healthy. There is a fairly substantial number of would-be entrants and a selection of those best suited for permanent employment is possible. Further, with the introduction of more science subjects into the curriculum of State schools and the progress of further education in pure and applied science for those young people, whether in town or country, who have recently left school, the next generation of entrants into agricultural labour and craftsmanship will have had a better school training than their forbears. Even if they do not—as some doubtless will—receive a farm institute training, they will have acquired sufficient knowledge of biological science or engineering to enable them to take an intelligent and co-operative interest in what goes on on the farm, and it is to be hoped that a fair proportion of them will eventually farm for themselves.

Conclusion

May I, in conclusion, briefly restate the simple thesis I have been endeavouring to make?

Whilst one must fight shy of exaggeration about national survival, there is no doubt that our country may find itself in an astonishingly uncomfortable, if not actually dangerous, position, unless more and better human food can be produced *per acre* from our own limited but potentially prolific acreage within the next very few years. We have gone some distance in the right direction, but we have still far to travel. The quality and efficiency of our agriculture though high are not yet high enough.

Successful farming in Britain has always been one of the most highly skilled of callings, demanding knowledge, judgment, forethought, technical skill and pertinacity. To-day, it needs and needs urgently the full application of recent scientific and engineering developments, and of modern methods of management. This is not merely a requirement to enable an ambitious farmer to get into the top stratum or to make more money, but has become a necessity for all farmers who are to pull their full weight in the struggle for the maintenance of our national economy.

My plea is, therefore, that for the improvement both of average quality in agriculture and the lasting benefit of our national economy, we should earnestly consider not only each and every way to eliminate inefficiency and complacency, but, from the positive side, how to introduce more rapidly into practice on the great majority of our farms the new knowledge that agricultural science has made available. Without another acre, or labour unit, or machine, there are great reserves of technological knowledge that could be thrown into the agricultural battle for increased home production. Research is undoubtedly needed *on the limiting factors which prevent the established results of agricultural research being sufficiently widely and effectively applied.*

One way to help quickly would be to assist into actual farming those scientifically trained as well as practically disposed young men anxious to farm but lacking the capital to begin. The full advantage to the farming community and the nation of our agricultural research findings and of the advisory work of the National Agricultural Advisory Service can be realised only if every farmer and farm worker has sufficient basic science to be able to understand and adapt the new information to improve his quality as farmer or craftsman and the production efficiency of his farm.

It seems very clear that the greater the extent to which, during the next year or two, we can apply the recent findings of agricultural science to the general improvement of farming quality the less hard will it go with us as a nation when, in 1952 or possibly a little later, the highly artificial and somewhat inglorious receipt of aid from abroad comes to an end.

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